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THE ANEROID BAROMETER, ITS VARIOUS FORMS, ITS THEORY AND ITS USE, WITH SPECIAL REFERENCE TO THE DETERMINATION OF ALTITUDES.

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I. GENERAL DISCUSSION.

The motor of all aneroids consists in an hermetically sealed box, the air of which has been exhausted as completely as practicable, and the flexible sides of which will consequently perform certain motions, when the pressure of the atmosphere changes. These motions are, however, too small to be perceived by the unaided eye, and they are, therefore, transmitted to a suitable mechanism, by means of which they are magnified to such an extent that they can be discerned and measured.

The invention of the aneroid dates back to the beginning of this century, one of the oldest forms being the one con-

structed by Bourdon, in which the box has the shape of a tube sealed at both ends, and bent into the shape of a circle. A small open space remains between the two ends, and diametrically opposite this gap the tube is fastened to a supporting plate. The cross-section of this tube is either rectangular or oval, and the direction of its greatest height or diameter stands at right angles to the plane of the circle into which the tube has been bent, which is parallel to the supporting plate.

When compressed air or steam is admitted into such a tube, the inner pressure thus created will have the tendency to straighten it, on account of the excess of pressure exercised on the *greater* area of the *outer* circular side as compared with the *smaller* area of the *inner* circular side. Any increase of the inner pressure will result in a further flattening of the circle into which the tube has been bent, while a decrease of it will cause such curve to be sharpened.

Precisely the reverse will take place, when the tube has been exhausted, because then the acting force, instead of being an *inside* pressure, will be the *outside* pressure of the atmosphere. The tube being held as described before at one point only, which is situated diametrically opposite the opening between the two ends, it is evident that the changes of form of the curve must result in movements of these ends; under the influence of high atmospheric pressure they will approach each other, while a decrease of pressure will widen the gap between them. The movements of the ends are transmitted by a simple arrangement to a hand pointing on a graduated dial. They are magnified either by levers acting on the *short* arm of the hand or pointer, or, when the latter is mounted centrally, by means of a toothed wheel and pinion.

This form of the aneroid has nowadays been entirely superseded by others, in which the vacuum box has the shape of a flat cylinder, into the upper and lower circular ends of which concentric grooves are pressed in order to equalize the motions performed by them under the influence of the varying atmospheric pressure. To the side of the vacuum box is soldered a small tin pipe, through which the

air is exhausted. After this has been done the pipe is sealed and the upper and lower ends of the box are now deflected towards its interior by the pressure of the atmosphere, the amount of such deflections for a given pressure being dependent on the strength of the plates. This must be sufficient to prevent too great a departure from the horizontal positions, in order not to strain the plates beyond their limit of elasticity. The movements are, therefore, necessarily small; a fall of $\frac{1}{100}$ of an inch in the mercurial barometer corresponding to an approach of about $\frac{1}{20000}$ of an inch of the plates of the vacuum chamber.

II. THE NAUDET ANEROID.

To measure such exceedingly small quantities, one class of modern aneroids is provided with an ingenious mechanism invented by Vidi and later improved by Naudet, the main parts of which are shown in *Fig. 1*.

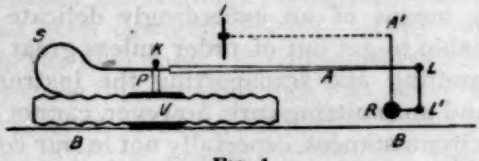

FIG. 1.

To the base or foundation plate *B B* are firmly attached a laminated spring *S* and the vacuum chamber *V*. The latter carries upon its centre an upright pillar *P*, which passes through an opening in the spring and presses on its upper side by means of the knife edge *K*. An elastic system is in this way formed by vacuum chamber and spring and the pulsations of the former will be imparted to the latter. The horizontal arm *A* is firmly attached to the spring and will therefore follow its movements. These will appear in a magnified scale at the end *L* and are transmitted by the two links *L* and *L'* to the rocker shaft *R*, which turns in bearings attached to the base plate, but not shown in the diagram. The turning motions of *R* are further magnified by the arm *A'* and thence transmitted by a small chain *C* to the central shaft *I*, which carries the index pointer. The required tension on the chain is produced by a spiral spring acting on

the central shaft. This spring and the bearings for the central shaft are not shown in the diagram.

The graduation of the dial on which moves the index pointer, like the hand on a watch dial, is made to correspond as nearly as possible with the reading of the mercurial barometer, and, like the latter, is expressed in millimetres or in inches. Perfect accuracy in this respect cannot be attained; there will always be required a certain correction for graduation. This and the corrections for temperature are the most important ones for practical work and will be discussed further below.

As apparent from the above, the use of the Naudet aneroid is very simple; in fact, the handling and reading of no instrument could be simpler. But this advantage is obtained by means which, in themselves, constitute the source of undoubted defects. The magnification and the measuring of the small movements of the vacuum chamber are performed by means of an exceedingly delicate mechanism, which is liable to get out of order unless great care is exercised in handling and transporting the instrument. Such constant and unremitting care, however, cannot be exercised under all circumstances, especially not in our country where the aneroid, in the hands of the railroad engineer for instance, is frequently subjected to swift transportation on horseback over many miles of rough country, the jolting and jarring of which are almost certain to prove injurious to the instrument. Even ordinary transportation in good packing frequently puts a Naudet out of working order, and no dealer in these instruments can warrant their safe arrival at the end of a long railroad journey.

Hence the frequent complaints of engineers, who have bought instruments in New York or some other great centre and to their dismay find them giving out completely when needed, a calamity which will be all the more annoying, when it happens, as it naturally often does, at a place far removed from any facilities for repairing.

Aside from such heavy shocks, which may at once render a Naudet perfectly useless, an instrument actually used in the field is necessarily exposed to small shocks, which even

the most careful and experienced observer cannot entirely avoid, and the continuance of which tends to produce a slackening of the mechanism. The gradual deteriorating from this cause of Naudet aneroids, during their use in the field, is a well-established fact, even when they are in the hands of the best observers (Report of Mr. J. Campbell, R. N., published in *U. S. Monthly Weather Review*, September 1879).

Finally a very important defect should be mentioned here, which may not inadequately be termed the "elastic reaction" of the instrument and which asserts itself in the tardiness with which the mechanism accommodates itself to changes in atmospheric pressure.

C. Kroeber, in his experiments with Naudet aneroids (*Zeitschrift für Vermessungswesen*, Heft. 8, 1881), has found that it took the instrument about two days to accommodate itself to a change of pressure corresponding to five inches of the mercurial column, the readings taken immediately after the pressure had been changed differing nearly two-tenths of an inch from the final ones. If not considered in the case of a measurement of altitudes this difference would have constituted an error of about 200 feet in about 5,000 feet.

It is true that the quality of "elastic reaction" in a measure is common to all aneroids, but it is especially pronounced in the Naudet, on account of its peculiar and complicated mechanism. As apparent from the above, the latter circumstance constitutes the origin of several serious defects, the only remedy for which, evidently consists in a simplification of the mechanism.

But it is as evident that such a simplification could not be attained, without at the same time sacrificing in a measure the ease and simplicity of handling and reading the instrument. Some of the work, which in the Naudet aneroid is performed by the complicated mechanism itself, in an instrument of simpler construction necessarily had to devolve on the observer. The difficulty then, consisted in harmonizing, in the most advantageous manner possible, requirements, the fulfilment of one of which impaired the fulfilment of the other.

III. THE GOLDSCHMID ANEROID.

This problem was solved after years of experiments by J. Goldschmid, of Zurich, about 1860, through an entirely novel construction, in which the movements of the vacuum chamber are measured by a micrometer screw acting on a peculiar lever arrangement, combined with optical magnification. In the following diagram, the left-hand side of which represents a section through the instrument, the exceedingly simple arrangement of the working parts is clearly shown.

To the centre of the top of the vacuum chamber *A A* is soldered a crank-shaped piece, which ends at one extremity in an upward turned knife edge; the latter supports the main lever *e e''*, which in its turn swings freely around the fulcrum *e''*. The movements of the vacuum chamber are

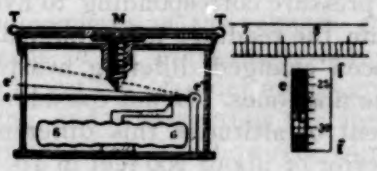

FIG. 2.

transmitted by the supporting knife edge to the main lever, the end *e* of which carries a small hammer-shaped piece. On the face of this is marked a horizontal index line, which will move vertically up and down, according to the varying pressure of the atmosphere. These movements take place alongside of a vertical ivory scale, which serves to measure them and which bears a graduation corresponding to the full inches of the mercurial column (see the right-hand side of *Fig. 2*, which shows the hammer and scale in front view).

While from the ivory scale the full inches only of the aneroid reading can be taken, the following arrangement serves for measuring the tenths and hundredths of inches. A fine and light spring *e' e''* is soldered to the main lever near the end *e''*; at the other end *e'* it carries a small hammer, similar to the one described before; when no pressure is exercised on the spring this hammer is held by the elas-

ticity of the same a little above the top of the hammer *e*, standing at the same time sideways of it.

To the cover *T T*, of the brass case enclosing the entire apparatus, is attached the micrometer screw *M*, which can therefore be screwed up or down by turning such cover. The lower pointed end of the micrometer acts on the spring *e' e''*, and when a reading is to be taken it is screwed down until the spring has been depressed so much that the index line of *e'* coincides with the index line of *e*.

It is at once clear that this position of the two hammers, which is shown in the diagram, corresponds to a certain distance between the point of the micrometer screw and the main lever, a distance which remains the same for all the various positions into which the main lever may be raised or lowered by the movements of the vacuum box. The point up to which the micrometer screw has to be screwed down, in order to make the two index lines coincide, is thus made a measure of the position in altitude of the main lever. This measure is read by means of a graduation engraved on the circumference of the cover and dividing the same into 100 equal parts, each of which corresponds to one hundredth of an inch of mercurial barometer height. The graduation is so wide that thousandths can easily be estimated.

The *modus operandi* then, in using the Goldschmid aneroid, is as follows:

The outer morocco case of the instrument is opened, and holding the latter in a horizontal position the cover is screwed down, while at the same time the two little hammers, protruding from the brass case through a window-like opening, are observed by means of the attached microscope. When the upper hammer is seen to commence moving downward, then the micrometer screw should be turned carefully and slowly, and when the two index lines coincide it is stopped entirely. Before such coincidence is obtained the instrument should be tapped lightly on the top, in order to eliminate any inertia of the mechanism. It is very important that this tapping should always be done in the same position of the hammers, because if, for instance, it is

done sometimes before coincidence and at other times at coincidence, then differences in the readings will be the consequence, that may amount to as much as $\frac{1}{100}$ of an inch. In order to insure uniformity in this respect it is advisable to make it a standing rule, *to tap when the lower edge of the upper hammer arrives at coincidence with the index line;* thence forward the screw should be turned very carefully, avoiding all further shocks.

IV. DISCUSSION OF THE CORRECTIONS.

In order to find, from the reading obtained by a Naudet or a Goldschmid aneroid, the height of the mercurial column of 32° Fahrenheit temperature, corresponding to the same pressure, three corrections are required, viz: for temperature, for graduation and for position, the latter not being required when the instrument is used for measuring altitudes only.

Let A represent the reading of the aneroid, T the temperature of the instrument in degrees Fahrenheit, and A_0 the reading that would have been obtained if the temperature had been 32° Fahrenheit, then we have, concerning the correction for temperature:

$$A_0 = A + a\,F(t) \qquad (1)$$

wherein a is the coefficient for temperature, the structure of the function t for the present remaining undecided.

Regarding the correction for graduation, aneroids are generally set so, that at a reading of thirty inches there is coincidence between them and the mercurial barometer, after the correction for temperature has been made on both. The coefficient of graduation is the difference between one inch of the mercurial column and one inch of the aneroid graduation: this—multiplied by the difference, thirty—reading, clearly renders the correction for graduation. Hence we have

$$B_0 = A_0 + \beta\,(30 - A_0)$$

wherein B_0 the height of the mercurial column reduced to the freezing point and β the coefficient for graduation.

The above described coincidence between the aneroid and the mercurial at thirty inches, even if completely attained, rarely lasts long, and in order to compensate for this a constant must be added to the right-hand side of the above equation. This constant is the correction for position and is generally designated by the letter C; we therefore have finally

$$B_0 = A_0 + \beta\,(30 - A_0) + C \qquad (2)$$

Proceeding now to discuss the structure of the above function of t, equation (1), it should be remembered that each change in temperature causes two different forces to act on the mechanism. In the first place, the corrugated surface of the vacuum chamber having a greater area than corresponds to its circumference, is expanded too much by a rise in temperature, and this excess of expansion causes a *depression* of the surface. The reason why the latter is not *raised* instead of being *depressed* is to be found in the circumstance, that in all good instruments it is bent slightly towards the interior, so that, even under a minimum of atmospherical pressure, it will only rise about to the horizontal, but never bulge outward beyond this.

The second force is created by the small quantity of air, present in all so-called vacuum chambers, being expanded by the additional heat imparted to it, thus exercising an inside pressure, that tends to counteract the first described force.

In the Naudet aneroid the chamber is exhausted as much as possible, and the inside pressure caused by a rise of temperature under all ordinary circumstances is therefore much smaller than the force caused by surface expansion of the box and pressing such surface towards the interior. These instruments will therefore be affected by a rise of temperature in the same way as by an increase of atmospherical pressure, that is, the reading of the instrument will become higher. Hence the correction for temperature in Naudet aneroids is subtractive for all temperatures above the freezing point and additive for all temperatures below.

From what has been said, it is clear that the second or inside force can be increased by admitting additional air

into the vacuum chamber, and it was suggested by Professor Kohlrausch, that in this way the two forces might be made to counterbalance each other entirely, thus reducing the correction for temperature to nothing. The impossibility of this was demonstrated by Professor Weilenmann, whose extensive theoretical and experimental researches on this subject rendered the following results:

(1) When the temperatures are plotted as ordinates and the corrections pertaining to them as abscisses, then a parabola is obtained.

(2) The apex of this parabola can be shifted to lower or higher temperatures, by respectively increasing or diminishing the amount of air contained in the chamber.

Although these principles differ theoretically from Bauernfeind's opinion, that the correction for temperature increases in direct proportion to the temperature, yet for Naudet aneroids, with highly exhausted chambers, there is no great practical difference between both assertions. This will be seen clearly from the following diagram, in the left-hand part of which a correction curve for temperature is shown, that was obtained by Professor Weilenmann from actual tests with an aneroid, the chamber of which was exhausted as completely as possible. For all such temperatures as are likely to occur in practical work, this curve does not depart materially from a straight line, which latter would be the expression of Bauernfeind's law in the diagram.

TEMPERATURE IN DEGREES CELSIUS.

Professor Weilenmann's above named principles have been utilized in the construction of the Goldschmid aneroid for the purpose of reducing as much as possible the correction for temperature. An amount of air corresponding to a few inches of the mercurial column is admitted into the vaccuum chamber, thereby shifting the apex of the parabola to near the freezing point and consequently reducing to practically nothing the corrections in that vicinity. An example of this is given in the right-hand part of *Fig. 3*, where a correction curve is shown for a vacuum box, which contained an amount of air corresponding to six inches

of the mercurial column. The characteristic differences between this curve and the high vacuum curve in the left-hand part of the same diagram, are at once apparent and strikingly to the advantage of the former. Under the influence of high temperatures the inner pressure now exceeds the force, bending the surface towards the interior, in other words, such influence will act on the Goldschmid aneroid like a *decrease* of atmospheric pressure.

Summing up, we find that the correction for temperature for the Goldschmid instruments in the vicinity of the freezing point is practically nothing, again, that as a general rule it is everywhere considerably less than for the Naudet aneroid or for the mercurial barometer, and, finally, that what little there is of it, is *additive*.

While the Goldschmid aneroid is not claimed to be entirely compensated for temperature, the corrections for the latter are doubtless in it reduced to very small quantities, a table of which, resulting from careful tests made by the manufacturer, accompanies every instrument.

For some of the Naudets in the market, entire compensation is claimed, but if the practical test is made, probably one out of ten will be actually found so, while the corrections of most of the rest will exceed those of the Goldschmid, without the purchaser being furnished with a table for them.

A simple and very effective method of ascertaining the correction for temperature consists in the use of warm, tepid and cold water baths, and finally the ice bath. By their means the inner temperature of an instrument can be varied from the freezing point upward, to say 100° or more. The observations must of course be made together with observations on a mercurial, and both are recorded in about the following way:

Mercurial Thermometer.	Aneroid.			
B_0	A	t	A'	$29{\cdot}094 - A'$
1	2	3	4	5
29·149	29·073	67·0	29·016	0·078
29·148	29·070	66·2	29·014	0·080
29·143	29·073	75·0	29·022	0·072
29·139	29·074	74·4	29·035	0·059
etc.	etc.	etc.	etc.	etc.

In this table the letters at the heads of the various columns have the following meaning:

(1) B_0 is the reading of the mercurial barometer in inches and decimals, as reduced to the freezing point.

(2) A is the reading of the aneroid under the same pressure.

(3) t is the inner temperature of the aneroid in degrees Fahrenheit and decimals.

(4) A' is what the aneroid would have read, if the pressure during the entire series of observations had constantly remained at $B'_0 = 29{\cdot}094$ (this being the average of all the B_0 of the entire series).

The value of A' is found in the following manner: In the case of the actual example cited here a preliminary determination had been made, showing that 1·000 inch of the aneroid corresponded to 0·970 inch of the mercurial column. If, therefore, in the case of the first observation for instance, the mercurial barometer had read 29·094 (instead of 29·149), that is 0·055 less than it actually did read, then the aneroid would clearly have read $\frac{1{\cdot}00}{0{\cdot}97} \times 0{\cdot}055 = 0{\cdot}057$ less than it actually did read, that is to say $29{\cdot}073 - 0{\cdot}057 = 29{\cdot}016$. The difference between the value $29{\cdot}094 - A'$ as found for the freezing point and as found for any other temperature renders the correction for such temperature. These differences should be found for the various temperatures from the freezing point upward to say 100° F., and then plotted, as shown in *Fig. 3;* from the curve thus obtained a table of corrections is easily deduced.

The correction for graduation has been assumed in equation (2) to stand in direct proportion to the difference 30.—Reading, an assumption, which although not strictly correct is sufficiently so for most practical purposes.

Each Goldschmid aneroid being furnished with a table of corrections for graduations, as well as for temperature, the owner of one of these instruments is saved the trouble of investigations in this respect.

For Naudet aneroids these tables have to be obtained by the purchaser, through direct comparison of the aneroid with the mercurial barometer, the readings of both being reduced to the freezing point by means of the previously

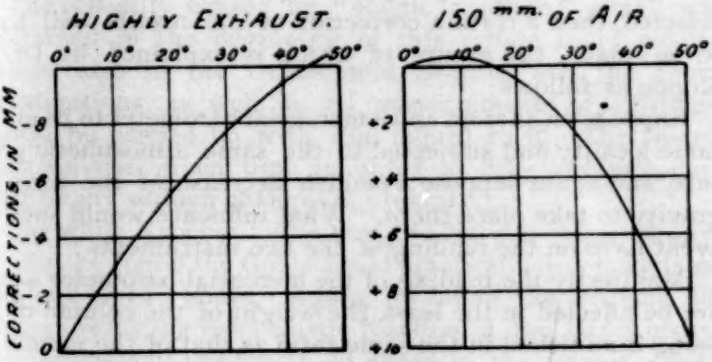

FIG. 3.

determined correction for temperature. The observations are recorded in tabulated form as follows:

B_0	A	t	A_0	A_0-B_0
29·970	29·960	72	30·000	0·030
29·420	29·450	76	29·500	0·080
28·870	28·940	78	29·000	0·130

wherein the various letters have the same meaning as in equations (1) and (2). From the table it is seen that in this case the constant correction for position was

$$C = -0{\cdot}03$$

and that 1·000 inch of the aneroid correspond to 1·100 inch

of the mercurial; the coefficient of graduation therefore was

$$\beta = -0{\cdot}100$$

and by means of these figures a table of corrections for graduation can easily be worked out according to equation (2).

But if it is desired to use the aneroid at great altitudes, then it would not be safe to assume the coefficient β to remain constantly the same, and the investigation should in this case be extended to lower pressures, either by making comparisons between the mercurial barometer and the aneroid at various altitudes, or by making them by means of the air-pump. If the latter method, however, is selected, then a certain correction of the results will have to be made, the nature of which is explained by Dr. C. Koppe as follows:

Suppose an aneroid and a mercurial barometer to be at the same locality and subjected to the same atmospheric pressure, and again suppose a sudden decrease of the force of gravity to take place there. What influence would such an event have on the reading of the two instruments?

Manifestly the reading of the mercurial barometer would not be affected in the least, the weight of the column of air being diminished in the same ratio as that of the mercurial column.

It would be quite different, however, with the aneroid. While the weight of the column of air has diminished, the elastic force of the vacuum chamber has remained the same; the instrument will therefore record a lower pressure.

Consequently, if a journey is undertaken with an aneroid and a mercurial barometer, which read precisely alike, then the coincidence between both will only last as long as the force of gravity remains unchanged. But this force is different in different latitudes, and at different altitudes, and from the latter circumstance results the necessity of a correction of such tables for graduation, that were obtained by means of the air-pump.

It is clear from the above that a reading of twenty-four inches of the mercurial barometer, taken at the level of the

sea under an air-pump, indicates a *greater* actual pressure of air than the same reading taken at an altitude of 7,000 feet above the level of the sea. Hence, a correction has to be *added* to the reading of the aneroid at 7,000 feet altitude, in order to obtain coincidence of such reading with the mercurial barometer. The amount of this correction has been computed by Professor Weilenmann as follows:

Reading (inches),	31·5	28·0	24·0	20·0	16·0
Correction, . . .	0	+0·105	+0·189	+0·270	+0·312

The arithmetical sums of these figures and the corresponding corrections found by means of the air-pump are the final corrections for graduation.

The difficulty caused by "elastic reaction," which was mentioned in the beginning of this article is, of course, present also in the Goldschmid aneroid, and the above investigations, as well as all measurements of altitudes, should be carried on with due regard to it. The instrument must be given time in order to fully accommodate itself to any sudden changes of pressure.

But while Kroeber found that the Naudet required days for this, he found the Goldschmid practically accommodated after one or two hours. Again, he found the extreme differences resulting from elastic reaction in the latter instrument only about one-fourth of those in the former. Both these advantages of the Goldschmid, as compared with the Naudet, are doubtless due to the extreme simplicity of the former's mechanism, and in view of them it may be safely asserted that the gradual changes of pressure, taking place during ordinary explorations of mountainous country, will not produce any appreciable errors of elastic reaction in the Goldschmid aneroid.

V. THE DETERMINATION OF DIFFERENCES OF ALTITUDE BY MEANS OF THE ANEROID BAROMETER.

The measuring of altitudes by means of the barometer, is mainly based on two suppositions, viz: Firstly, that the atmospheric strata of equal pressure are horizontal, and secondly, that the temperature of a vertical column of air is

equal to the arithmetical mean of the temperature observed at its top and at its bottom.

These two conditions are probably *never* fulfilled completely in nature, but they are *always* fulfilled more or less approximately. On the greater or smaller deviation from them of the actually existing conditions, depends the exactitude of the result in each case, leaving out the consideration, of course, of avoidable errors of observations.

In accordance with the two principles mentioned, the process of barometric measurement may be described thus: By means of the barometer, be it a mercurial or be it an aneroid, the *weight* of a column of air is determined, and from the *observed temperature* of such column its length is found.

During observations for altitude, therefore, the temperature of the *air* as well as that of the *instrument* must be observed; the first one for the purpose named just now, the last one for the purpose of taking into proper account the corrections for temperature, by reducing both observations to uniform temperature. Besides this, the observations made at the upper and at the lower stations must of course be corrected to graduation, so that thus their difference is made equal as nearly as possible to the difference that would have been obtained by using a mercurial barometer.

It is clear at once that the observations at the upper and lower stations should be made simultaneously in order to obtain both as nearly as possible under the same atmospheric conditions. Observations made by one observer, in passing from point to point with his instrument, are quite unreliable, unless he returns to the previous point after each observation in order to take a second reading there, and thus to find the changes that have taken place in pressure and temperature. Only by means of this tedious and time-robbing procedure, can anything like fair results be obtained by one observer.

For serious work then, two observers, and at least two instruments are required, and the readings of the latter, as well as the outer and inner temperature, should be recorded with great exactitude, so as to be enabled to subsequently

introduce the proper corrections, without which the results are worthless. Great care also should be taken not to expose the aneroid to the direct rays of the sun when taking an observation, but to let it assume as nearly as possible the temperature of the surrounding air in the shadow.

The *modus operandi* in determining differences of altitude may be described about as follows:

A stationary instrument, with an observer, is placed at some point of known altitude that has been selected as the basis of operations. This instrument is observed regularly every fifteen or thirty minutes, and a careful record kept of such readings, as well as of the time and temperature corresponding to each.

Meanwhile, the engineer travels through the district he intends to survey, taking observations wherever he sees fit, and keeping a careful record of their location, time, temperature, etc. When he is through with his work, an observation at the point of known altitude is computed for each of his observations, from the record kept by the assistant; this having been done, the difference of altitude between each point of observation and the point of known altitude is obtained in a simple way, by means of the subjoined table No. 1, which has been taken from *Meteorological and Physical Tables of the Smithsonian Institution.* [Other convenient tables are given in *The Aneroid Barometer*, Van Nostrand, New York, 1888, and in the above mentioned publication of the Smithsonian Institution.]

For instance, suppose 29·5 to have been the reading at the point of known altitude, and 26·5 the reading taken by the engineer, both corrected to graduation, etc., and reduced to 32° F., and again, suppose 70° and 60° to have been the respective temperatures of the surrounding air, then we obtain from the table:

For 29·5 inches and 70° F., ...	96·5
For 26·5 inches and 60° F., ...	105·3
Mean, ...	100·9

This mean, multiplied by the difference of the two read-

ings expressed in tenths of an inch, renders the difference in altitude:

$$30 \times 100{\cdot}9 = 3{,}027 \text{ ft.}$$

The use of this table gives results of sufficient accuracy for all practical purposes, so that recourse to the complicated barometrical formula is not required.

Readings should always be taken in the same position of instrument, preferably the horizontal one.

TABLE No. 1.

HEIGHT IN FEET OF A COLUMN OF AIR CORRESPONDING TO ONE-TENTH OF AN INCH IN THE BAROMETER.

Barometer Reading in Inches.	Temperature of Air in Degrees Fahrenheit.					
	40°	50°	60°	70°	80°	90°
23·0	116·2	118·8	121·3	123·8	126·4	129·9
23·5	113·7	116·2	118·7	121·2	123·7	126·1
24·0	111·3	113·8	116·2	118·6	121·1	123·5
24·5	109·1	111·5	113·8	116·2	118·6	121·0
25·0	106·9	109·3	111·6	113·9	116·3	118·6
25·5	104·8	107·1	109·3	111·6	113·9	116·2
26·0	102·7	105·0	107·2	109·5	111·7	114·0
26·5	100·9	103·2	105·3	107·5	109·7	111·8
27·0	99·0	101·2	103·3	105·5	107·6	109·8
27·5	97·2	99·3	101·4	103·5	105·6	107·8
28·0	95·4	97·5	99·6	101·7	103·8	105·9
28·5	93·8	95·8	97·9	99·9	101·9	103·9
29·0	92·1	94·1	96·2	98·2	100·2	102·2
29·5	90·6	92·6	94·5	96·5	98·5	100·4
30·0	89·1	91·0	92·9	94·9	96·8	98·8
30·5	87·6	89·5	91·4	93·3	95·2	97·2

TABLE No. 2.

REDUCTION OF MERCURIAL COLUMN TO 32° F., BRASS SCALE TO BAROMETERS CORRECT AT 62° F

Temperature.	Reading of Barometer.		
	30 inches.	25 inches.	20 inches.
32°	·009	·008	·006
35°	·017	·015	·012
40°	·031	·026	·021
45°	·044	·037	·030
50°	·058	·048	·038
55°	·071	·059	·047
60°	·084	·070	·056
65°	·098	·082	·065
70°	·111	·093	·074
75°	·125	·104	·083
80°	·138	·115	·092
85°	·151	·126	·101
90°	·164	·137	·110
95°	·178	·148	·118
100°	·191	·159	·127

The corrections contained in this table have to be *subtracted* from the reading of the mercurial barometer, in order to reduce the same to a temperature of 32° F.

CAPTAIN ABNEY ON HELIOCHROMY.

By F. E. IVES.

Captain Abney, in a recent address before a section of the British Association*, made some brief remarks upon the subject of heliochromy, which are, in my opinion, so misleading as to call for correction. I quote as follows:

"The nearest approach to success in producing colored pictures by light alone is the method of taking three negatives of the same subject through different colored glasses, complementary to the three color-sensations, which together give to the eye the sensation of white light. The method is open to objection on account of the impure color of the glasses used. If a device could be adopted whereby only those three parts of the spectrum could be severally used which form the color-sensations, the method would be more perfect than it is at present. Even then, perfection could not be attained, owing to a defect which is inherent in photography. This defect is the imperfect representation of gradation in tone."

According to those recent text-books on color which I have seen, only such light rays as are supposed to affect only one kind of nerve fibrils in the eye, or to excite only one of the fundamental color-sensations, can be said to form or represent primary color-sensations, and such rays are confined to both ends and a narrow strip in the middle of the visible spectrum.† If Captain Abney means to assert that in a process of this character only those rays of the spectrum should act, which represent primary color-sensations, he is certainly mistaken, and grievously misleading all those who accept him as an authority upon this subject.

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* Reprinted in the *Scientific American Supplement*, Oct. 5, 1889, p. 11,472.

† "Helmholtz, Maxwell and Rood, as well as many other physicists, have developed the theory of Wunsch and Young, and have adopted the same, or very nearly the same, trial of primary color-sensations. These fundamental hues or primaries * * represent three widely separated and very bright colors of the spectrum."—*Color*, by A. H. Church, M. A. London, 1887. p. 67. See also Rood's *Modern Chromatics*, pp. 120–23.

It is certain that every ray of the visible spectrum should act, and act nearly in proportion to its power to excite the sensation of light in the eye. It would be ridiculous to expect that a process which reproduced the spectrum as three detached and widely separated patches of color would correctly reproduce the infinite variety of compound colors in nature, some of which are made up chiefly, and most of them partly, of rays lying in other parts of the spectrum. There are some moderately bright colors which would reproduce like black by such a process.

But it is not easy for me to believe that Captain Abney means what I have inferred from his statement, although the expressions "parts of the spectrum" and "impure color of the glasses" certainly support this inference. When he says, "those parts of the spectrum * * which form the color-sensations," it is possible that he may mean the light rays in proportion as they affect the different kinds of nerve fibrils or excite the different primary color-sensations in the eye. If so, he does not mean to divide the spectrum into three distinct parts, but aimed to state a fact which was first observed by me, and plainly set forth in my book, *A New Principle in Heliochromy*, where I showed that each heliochromic negative must be made by the joint action, in due proportion, of all rays which affect the primary color-sensation which it represents. It does not require screens of "pure color" to do this—quite the contrary. Screens of "pure color" are screens transparent to single regions of the spectrum only, and would not transmit all the rays that affect a single kind of nerve fibrils in the eye. The red, orange, yellow and yellow-green rays all affect the nerve fibrils which produce the red color-sensation, and all of them must therefore be transmitted more or less freely by the color screen used in making the negative representing that primary color-sensation. But the orange-yellow and yellow must also be transmitted to some extent by the screen used in making the negative representing the green color-sensation.

The screens must, in short, be so graduated in color as to secure, in negatives of the spectrum, curves of intensity like

the curves of a diagram representing the action of the spectrum upon the three kinds of nerve fibrils in the eye, or upon the three fundamental color-sensations; and it is a fact that such screens had been produced and that correct heliochromic negatives had been made in considerable number, and with ease and certainty, before the beginning of this year.

The observation that the inability of photography to exactly represent gradations in tone would affect the accuracy of such a method of reproducing colors, of which Capt. Abney now makes so much, originated with myself; but I also showed that by my method of proving the color screens by reference to the curves of intensity in the spectrum negatives, the effect of this defect in the negative process could be so far compensated for that it would no longer seriously affect the result, if suitable sensitive plates were employed. To say that this method does not solve the problem because of this defect, would be equivalent to saying that photography has not solved the problem of reproducing light and shade.

A NEW GRAPHICAL METHOD OF OBTAINING PIER MOMENTS.

By C. H. Lindenberger.

A method of calculating strains graphically approaches perfection in just so far as it substitutes the mechanical labor of the draughtsman for the intellectual labor of the analyst. In the case of the continuous girder, there is no method that I have heard of (except the method about to be developed) of obtaining the pier moments without a great amount of analytical labor with its constant danger of gross errors and mistakes that seriously detract from its merits as a practical method of obtaining strains. This is unfortunate, for it is often necessary to know the value of the strain on a structure whose cost is measured by hundreds instead of millions of dollars, but all previous methods require an amount of labor that is practically prohibitory.

With these preliminary remarks I shall proceed to develop a purely graphic method of obtaining the pier moments of a continuous girder, that is free from almost all the objections to previous methods, being short and simple in its practical application.

Let us first begin by a few definitions.

Let the girder have s spans, and, therefore, $s + 1$ supports.

Let l_n be the length of the n^{th} span from the left.

Let M_n be the moment at the n^{th} support.

Let EI be the product of the constant modulus of elasticity by the constant moment of inertia.

Let P_n be a weight in the n^{th} span.

Let a be its distance from the nearest left support.

Let C_n and d_n be indeterminate multipliers by the use of which the pier moments are obtained.

Let Z_s be a constant in terms of these multipliers.

Let Y_n be a term dependent on the loading and heights of the supports.

Then in a previous article in December, 1888, of this JOURNAL, I demonstrated the following theorem:

$$M_n = \frac{\left\{\begin{array}{c} d_n(C_2Y_2 + C_3Y_3 + etc. + C_nY_n) + \\ + C_n(d_{n+1}Y_{n+1} + d_{n+2}Y_{n+2} + etc. + d_sY_s) \end{array}\right\}}{Z_s} \quad (1)$$

The equations from which the value of the indeterminate multipliers are found have the general form

$$C_{n-1}\,l_{n-1} + 2C_n\,(l_{n-1} + l_n) + C_{n+1}\,l_n = 0 \quad (2)$$

$$d_{n-1}\,l_{n-1} + 2d_n\,(l_{n-1} + l_n) + d_{n+1}\,l_n = 0 \quad (3)$$

The assumption being made that $C_1 = 0$, $C_2 = 1$, $d_{s+1} = 0$, $d_s = 1$.

The theorem of three moments is:

$$M_{n-1}\,l_{n-1} + 2M_n\,(l_{n-1} + l_n) + M_{n+1}\,l_n = Y_n \quad (4)$$

The value of Z_s is to be found from either of the three following equations:

$$C_{s-1}\,l_{s-1} + 2C_s\,(l_{s-1} + l_s) = Z_s \quad (5)$$

$$2d_2\,(l_1 + l_2) + d_3l_2 = Z_s \quad (6)$$

$$(C_n\,d_{n+1} - C_{n+1}\,d_n)\,l_n = Z_s \quad (7)$$

All these formulas were demonstrated the previous

article, December, 1888. Equations (1) and (7) are the basis upon which the graphical method is developed.

There are in each of the spans two fixed points, the definitions of which are given by

$$i_n = \frac{C_n l_n}{C_n - C_{n+1}} \tag{8}$$

$$I_n = \frac{d_n l_n}{d_n - d_{n+1}} \tag{9}$$

These formulas are given by Professor Merriman in his "Theory and Calculation of Continuous Girders," Van Nostrand *Science Series*, No. 25, the discrepancy in equation (9) being accounted for by the fact that I have numbered these d multipliers the other way.

Professor Dubois in his *Graphical Statics* gives the graphic method of finding these fixed points, but as the method is so practically important I repeat it and give the demonstration so that it may be thoroughly understood.

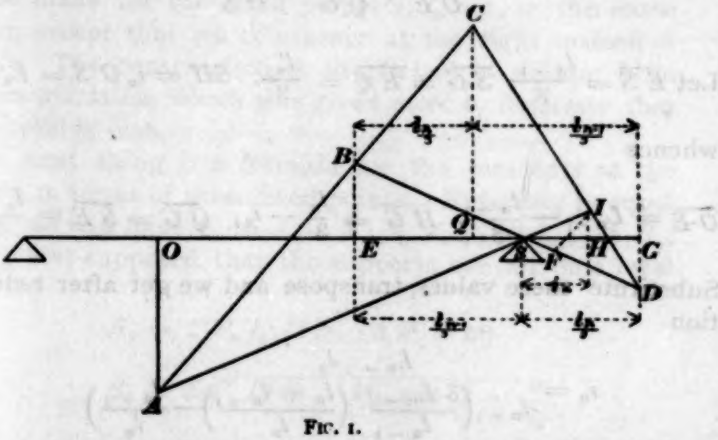

FIG. 1.

In *Fig. 1*, let s be the n^{th} support and let $\overline{I F}$ be parallel to $\overline{A C}$, $\overline{O G}$ being the line of supports when on a level. Then from the two similar triangles $\overline{I F D}$ and $\overline{C B D}$ we obtain

$$\overline{IF} = \frac{\overline{CB} \times \overline{ID}}{\overline{CD}}$$

also from the similar triangles $\overline{IFS}$ and $\overline{ABS}$ obtain

$$\overline{IF} = \frac{\overline{AB}\ \ \overline{SI}}{\overline{SA}}$$

the relations also exist

$$\frac{\overline{SI}}{\overline{SA}} = \frac{\overline{SH}}{\overline{OS}}$$

$$\frac{\overline{ID}}{\overline{CD}} = \frac{\overline{HG}}{\overline{QG}}$$

$$\frac{\overline{CB}}{\overline{AB}} = \frac{\overline{EQ}}{\overline{OE}}$$

and placing the two values of $\overline{IF}$ equal we obtain

$$\frac{\overline{EQ} \times \overline{HG}}{\overline{OE} \times \overline{QG}} = \frac{\overline{SH}}{\overline{OS}}$$

Let $\overline{ES} = \frac{l_{n-1}}{3}$ $\overline{SG} = \overline{EQ} = \frac{l_n}{3}$, $SH = i_n$ $OS = L_{n-1}$

whence

$$\overline{OE} = L_{n-1} - \frac{l_{n-1}}{3},\ \overline{HG} = \frac{l_n}{3} - i_n,\ \overline{QG} = \overline{SE} = \frac{l_{n-1}}{3}$$

Substitute these values, transpose and we get after reduction

$$i_n = \frac{L_{n-1}\, l_n}{l_{n-1}\left(\frac{3\, L_{n-1}}{l_{n-1}}\left(\frac{l_n + l_{n-1}}{l_n}\right) - \frac{l_{n-1}}{l_n}\right)}$$

and from equation (2) we obtain

$$\frac{l_{n-1}}{l_n} = -\frac{C_{n+1} + 2\, C_n}{C_{n-1} + 2\, C_n}$$

$$\frac{l_n + l_{n-1}}{l_n} = \frac{C_{n-1} - C_{n+1}}{C_{n-1} + 2\, C_n}$$

giving by substitution

$$i_n = + \frac{L_{n-1} l_n (C_{n-1} + 2 C_n)}{l_{n-1}\left(\frac{3 L_{n-1}}{l_{n-1}}\left(C_{n-1} - C_{n+1}\right) + (C_{n+1} + 2 C_n)\right)}.$$

To give a definite value to i_n we must assume a value for L_{n-1}, therefore assume

$$L_{n-1} = - \frac{l_{n-1} C_n}{C_{n-1} - C_n}$$

and this gives after substituting and reducing

$$i_n = \frac{C_n l_n}{C_n - C_{n+1}}$$

as it should, and therefore

$$L_{n-1} = l_{n-1} - i_{n-1}.$$

In like manner the construction and demonstration may be made for the fixed points I_1 I_2, etc., in the same manner, except that we commence at the right instead of the left. The construction is simple and is evident from the demonstration, which was given more to illustrate that than anything else.

The next thing is a formula for the moments at the supports in terms of these fixed points. Referring to equation (1), we will have $Y_n = A_n + B_{n-1}$, $Y_{n+1} = A_{n+1} + B_n$; it being first supposed that the supports are all on a level and

$$A_n = \Sigma P_n l_n^2 (2 k - 3 k^2 + k^3)$$

$$B_n = \Sigma P_n l_n^2 (k - k^3), \text{ and } k = \frac{a}{l_n}.$$

Here it would be proper to give a diagram for the determination of

$$\frac{A_n}{l_n^2} \text{ and } \frac{B_n}{l_n^2},$$

which is accordingly done in *Fig. 2*, where $\overline{O L}$ is taken to represent the span, and $\overline{O P} = P$ to a sufficiently large

scale, and the lines $\overline{GC}$, $\overline{HK}$, $\overline{DJ}$ and $\overline{PI}$ are parallel to $\overline{OL}$ and perpendicular to $\overline{OP}$.

Let $\overline{OA} = \overline{PB} = a = k\,l$, then $\overline{AL} = (1-k)\,l$, and $\overline{AF} = P(1-k) = \overline{LK} = \overline{OH}$. $\overline{CF} = P(1-k)^2 = \overline{GH}$ and $\overline{MF} = P(1-k)^3$; and therefore

$$\overline{AM} = \overline{AF} - \overline{MF} = P(1-k-(1-k)^3)$$
$$= P(2k - 3k^2 + k^3)$$

$\overline{FB} = Pk = \overline{HP} = \overline{KI}$ and $\overline{DF} = Pk^2 = \overline{KJ}$ and $\overline{FE} = Pk^3$, and therefore

$\overline{EB} = P(k - k^3)$.

Referring again to equation (1) and inserting the value of Z_s given by equation (7), let us suppose that only the n^{th} span is loaded.

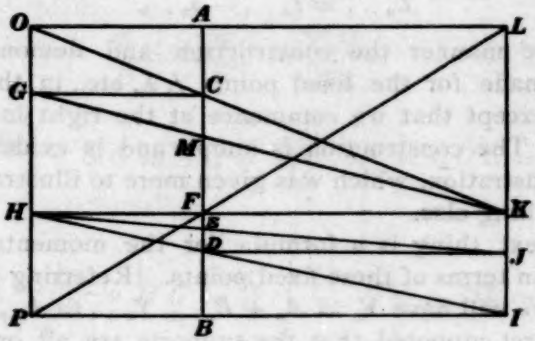

FIG. 2.

Then we have

$$M_n = \frac{d_n C_n A_n + C_n d_{n+1} B_n}{(C_n d_{n+1} - C_{n+1} d_n) l_n}$$

Substituting the values of i_n and I_n from equations (8) and (9), we obtain

$$M_n = \frac{I_n i_n \frac{A_n}{l_n^2} - i_n (l_n - I_n) \frac{B_n}{l_n^2}}{I_n - i_n} \qquad (10)$$

Referring again eq. 1, we write

$$M_{n+1} = \frac{\left\{ \begin{matrix} d_{n+1}(C_2 Y_2 + C_3 Y_3 + etc. + C_n Y_n + C_{n+1} Y_{n+1}) \\ + C_{n+1}(d_{n+2} Y_{n+2}, etc., etc., + d_s Y_s) \end{matrix} \right\}}{Z_s}$$

Supposing, as before, that only the n^{th} span is loaded, we have

$$M_{n+1} = \frac{d_{n+1} C_n A_n + C_{n+1} d_{n+1} B_n}{(C_n d_{n+1} - C_{n+1} d_n) l_n}$$

and substituting the value of i_n and I_n as before, we get

$$M_{n+1} = \frac{(l_n - I_n)(l_n - i_n) \frac{B_n}{l_n^2} - i_n (l_n - I_n) \frac{A_n}{l_n^2}}{I_n - i_n} \qquad (11)$$
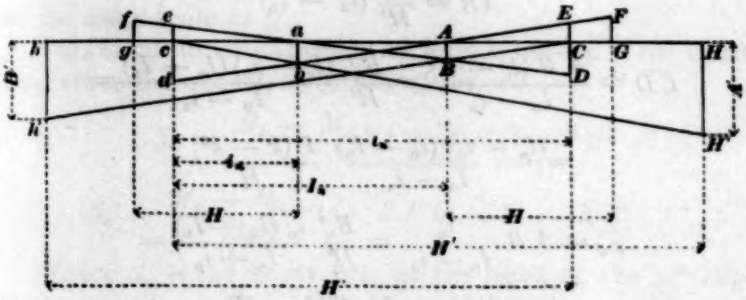
FIG. 3.

Subtracting this equation from equation (10) and dividing the result by l_n, gives

$$\frac{M_n - M_{n+1}}{l_n} = \frac{i_n \frac{A_n}{l_n^2} - (l_n - I_n) \frac{B_n}{l_n^2}}{I_n - i_n} \qquad (12)$$

Since

$$\frac{A_n}{l_n^2} \text{ and } \frac{B_n}{l_n^2}$$

we have seen may each be represented by a line, let

$$\frac{A_n}{l_n^2} = A_n' \text{ and } \frac{B_n}{l_n^2} = B_n'$$

$$\frac{M_n}{H} \text{ and } \frac{M_{n+1}}{H}$$

will also be represented by A line if H is a line representing a force, and will usually be taken equal to the pole distance of the force polygon.

When there are several weights on the span, it will be found in practice that P must be taken in *Fig. 2* to a con-

siderably larger scale than is convenient for the force polygon or shear line, because in this way cumulative errors are avoided to some extent.

In *Fig. 3* let $\overline{Cc}$ be the length of the n^{th} span and let $\overline{ca} = i_n$, $\overline{cA} = I_n$, $\overline{hC} = \overline{cH} = H'$, where H' is the length of a line representing H to the same scale that P is taken in *Fig. 2*. Lay off in the figure $\overline{HH'} = A_n'$ $\overline{hk'} = B_n'$ taken from *Fig. 2*, then

$$\overline{AB} = \frac{B_n'}{H'}(l_n - I_n)$$

$$\overline{CD} = \frac{\overline{AB}(l_n - i_n)}{I_n - i_n} = \frac{B_n'}{H'}\frac{(l_n - i_n)(l_n - I_n)}{I_n - i_n} =$$

$$= \frac{(l_n - i_n)(l_n - I_n)}{I_n - i_n}\frac{P(k - k^3)}{H}$$

$$\overline{ec} = AB\frac{i_n}{I_n - i_n} = \frac{B_n'}{H'}\frac{i_n(l_n - I_n)}{I_n - i_n} =$$

$$= \frac{i_n(l_n - I_n)}{I_n - i_n}\frac{P(k - k^3)}{H}$$

$$\overline{ab} = \frac{A_n}{H'}i_n,\ \overline{cd} = \overline{ab}\frac{I_n}{I_n - i_n} = \frac{A_n'}{H'}\frac{I_n i_n}{I_n - i_n} =$$

$$= \frac{I_n i_n}{I_n - i_n}\frac{P(2k - 3k^2 + k^3)}{H}$$

$$\overline{EC} = \frac{\overline{ab}(l_n - I_n)}{I_n - i_n} = \frac{A_n}{H'}\frac{i_n(l_n - I_n)}{I_n - i_n} =$$

$$= \frac{i_n(l_n - I_n)}{I_n - i_n}\frac{P(2k - 3k^2 + k^3)}{H}$$

and from the foregoing

$$\frac{M_n}{H} = \overline{cd} - \overline{ec}$$

$$\frac{M_{n+1}}{H} = \overline{CD} - \overline{EC}$$

Let $\overline{ag} = \overline{AG} = H$ to the scale of the pole distance in the force polygon.

Then

$$\overline{fg} = \frac{\overline{EC} \times H}{i_n} = \frac{P(k - k^3)(l_n - I_n)}{I_n - i_n}$$

also

$$\overline{FG} = \frac{\overline{ec} \times H}{(l_n - I_n)} = P(2k - 3k^2 + k^3)\frac{i_n}{I_n - i_n}$$

and therefore

$$\frac{M_n - M_{n+1}}{l_n} = \overline{FG} - \overline{fg}$$

to the same scale as H.

This is an important quantity, as it is needed in the shear line, since the equations for the shear are

$$S_n = \frac{M_n - M_{n+1}}{l_n} + \Sigma P(1 - k)$$

$$S_n' = \frac{M_n - M_{n+1}}{l_n} - \Sigma P k = S_n - \Sigma P$$

Where S_n is the shear just to the right of the nth support.

Having now found the moments at the nth and (n + 1)th support due to a load in the nth span the moments due to this load at any other support are easily found and the method is given by other writers but is nevertheless repeated here.

Let M_q be the moment desired, there being no load except on the nth span and the supports on a level

Then from Eq. (1) where $n > q$ we have

$$M_q = \frac{C_q}{C_n} M_n$$

or what is the same thing

$$M_q = \left(\frac{C_q}{C_{q+1}} \times \frac{C_{q+1}}{C_{q+2}} \times etc. \times \frac{C_{n-2}}{C_{n-1}} \times \frac{C_{n-1}}{C_n} \right) M_n$$

and from equation (8) this becomes

$$M_q = \left(\frac{i_q}{i_q - l_q} \times \frac{i_{q+1}}{i_{q+1} - l_{q+1}} \times etc. \times \frac{i_{n-2}}{i_{n-2} - l_n} \times \frac{i_{n-1}}{i_{n-1} - l_{n-1}} \right) M_n$$

In like manner from equation (1) where $(n+1) < q$ we obtain

$$M_q = \frac{d_q}{d_{n+1}} M_{n+1}$$

or the identical equation

$$M_q = \left(\frac{d_q}{d_{q-1}} \times \frac{d_{q-1}}{d_{q-2}} \times etc. \times \frac{d_{n+3}}{d_{n+2}} \times \frac{d_{n+2}}{d_{n+1}}\right) M_{n+1}$$

and from equation (9)

$$M_q = \left(\frac{I_{q-1} - l_{q-1}}{I_{q-1}} \times \frac{I_{q-2} - l_{q-2}}{I_{q-2}} \times etc. \times \frac{I_{n+2} - l_{n+2}}{I_{n+2}} \times \right.$$
$$\left. \times \frac{I_{n+1} - l_{n+1}}{I_{n+1}}\right) M_{n+1}$$

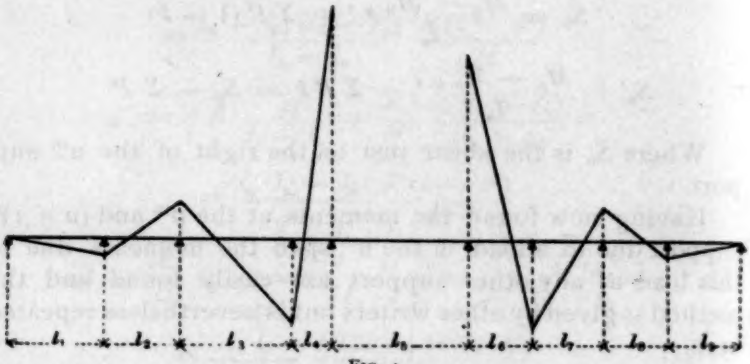

FIG. 4.

The graphical method of interpreting these equations is shown in *Fig. 4*, in which there are nine spans, but the fifth span only is loaded. At the fifth support the ordinate $= \frac{M_5}{H}$ and a line through its extremity and through the fixed point nearest the fourth support cuts off from the vertical through the fourth support a line $= \frac{M_4}{H}$, the operation is repeated for the next and so on, and a similar operation takes place on the right. If the supports are counted each way from the loaded span, the moments at the odd supports will be negative, and those at the even supports will be positive, a positive moment being one that causes tension on the upper chord or flange.

It will be observed that Eq. (8) gives $i_1 = o$, hence $L_1 = l_1$. Also from Eq. (9) $I_n = l_n$.

The construction for I_1, I_2, etc., etc., is easily found by remembering that these fixed points would not change their actual position if the order of the spans was reversed. Hence, the graphical construction is the same as for i_2, i_3, etc., etc., except that we begin at the right and work towards the left, instead of the opposite, as shown in *Fig. 1.* The obliquity of the line $\overline{A\,C}$ and the length of the line $\overline{A\,O}$ is arbitrary.

The next problem to attack is the moment due to the sinking of any one of the supports which can also be shown to be more easily solved graphically than analytically. In this case it is to be remembered that the girder is to be wholly unloaded, and these moments are just sufficient to compel contact with the supports.

For this case the well-known formula is—

$$Y_n = -6\,E\,I'\left\{\frac{h_n - h_{n+1}}{l_n} + \frac{h_n - h_{n-1}}{l_{n-1}}\right\}$$

Let all the supports be on a level, except the n^{th} support and this support be depressed *below* the level of the rest by the distance h.

Now, recollecting that this makes—

$$Y_{n-1} = 6\,E\,I'\,\frac{h}{l_{n-1}}$$

$$Y_n = -\frac{6\,E\,I'\,h}{l_n\,l_{n-1}}\,(l_{n-1} + l_n)$$

$$Y_{n+1} = 6\,E\,I'\,\frac{h}{l_n}$$

and that all the other $Y^s = o$, we can easily derive the following formulas, M_q being the moment sought:

for $q < n$

$$M_q = \frac{6\,E\,I'\,h}{Z_s}\,C_q\left\{\frac{d_{n-1} - d_n}{l_{n-1}} + \frac{d_{n+1} - d_n}{l_n}\right\} \qquad (13)$$

for $q = n$

$$M_q = M_n = \frac{6\,E\,I'\,h}{Z_s}\left\{\frac{d_n\,(C_{n-1} - C_n)}{l_{n-1}} + \frac{C_n\,(d_{n+1} - d_n)}{l_n}\right\} \quad (14)$$

for $q > n$

$$M_q = \frac{6\,E\,I'\,h}{Z_s}\,d_q\left\{\frac{C_{n-1} - C_n}{l_{n-1}} + \frac{C_{n+1} - C_n}{l_n}\right\} \quad (15)$$

In Eq. (13) make $q = n - 1$; and in Eq. (15) make it $= n + 1$; and take $Z_s = (C_n\,d_{n+1} - C_{n+1}\,d_n)\,l_n$; and substitute from Eqs. (8) and (9) and we obtain the following equations:

$$M_{n-1} = \frac{6\,E\,I'\,h}{l_n^2}\left\{\frac{i_{n-1}\,i_n\,(I_n + l_{n-1} - I_{n-1})}{(l_{n-1} - i_{n-1})(l_{n-1} - I_{n-1})(I_n - i_n)}\right\} \quad (16)$$

$$M_n = -\frac{6\,E\,I'\,h}{l_n^2}\left\{\frac{i_n\,(I_n + l_{n-1} - i_{n-1})}{(l_{n-1} - i_{n-1})(I_n - i_n)}\right\} \quad (17)$$

$$M_{n+1} = \frac{6\,E\,I'\,h}{l_n^2}\left\{\frac{(i_n + l_{n-1} - i_{n-1})\,(I_n - I_n)}{(l_{n-1} - i_{n-1})\,(I_n - i_n)}\right\} \quad (18)$$

By making $Z_s = (C_{n-1}\,d_n - C_n\,d_{n-1})\,l_{n-1}$; which is merely reducing the subscripts in Eq. (7) by unity we get by substituting the values of the fixed points:

$$M_{n-1} = \frac{6\,E\,I'\,h}{l_{n-1}^2}\left\{\frac{I_n + l_{n-1} - I_{n-1})\,i_{n-1}}{(I_{n-1} - i_{n-1})\,I_n}\right\} \quad (19$$

The only practical value of these formulas is to obtain a diagram for the graphical calculation of moments at the supports which I will now proceed to explain.

In *Fig. 5*, let A be the $(n-1)^{th}$ support, D the n^{th} support and G the $(n+1)^{th}$ support. B and C are the fixed points in the span l_{n-1} and E and F those in the span l_n.

Let H be any arbitrarily assumed force, then lay off in a vertical through D a line $=$

$$\frac{6\,E\,I'\,h}{H\,l_n^2} = \overline{D\,p} = p$$

say, and through its extremity draw $\overline{B\,f}$ and drop vertical $\overline{c\,C}$, $\overline{e\,E}$ and $\overline{F\,f}$, then

$$\overline{F\,f} = p\,\frac{(I_n + l_{n-1} - i_{n-1})}{l_{n-1} - i_{n-}}$$

produce the vertical $\overline{D\,p}$ below the line of supports, and draw $\overline{f\,d}$ through E, cutting this vertical at d, then

$$\overline{D\,d} = \frac{\overline{F\,f}\,i_n}{I_n - i_n} = p\,\frac{(I_n + l_{n-1} - i_{n-1})\,i_n}{(l_{n-1} - i_{n-1})\,(I_n - i_n)} = -\frac{M_n}{H}$$

We also have

$$\overline{E\,e} = p\,\frac{(i_n + l_{n-1} - i_{n-1})}{l_{n-1} - i_{n-1}}$$

Draw $\overline{e\,g}$ through F, cutting the vertical through G at g, then

$$\overline{G\,g} = \frac{\overline{E\,e}\,(l_n - I_n)}{I_n - i_n} = \frac{p\,(i_n + l_{n-1} - i_{n-1})\,(l_n - I_n)}{(l_{n-1} - i_{n-1})\,(I_n - i_n)} = \frac{M_{n+1}}{H}$$

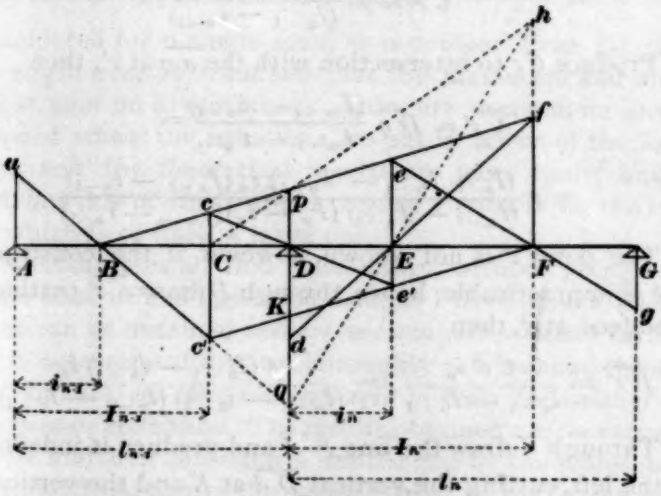

FIG. 5.

Through p draw $\overline{C\,h}$, cutting the vertical $\overline{F\,f}$ produced at h, then

$$\overline{F\,h} = p\,\frac{(I_n + l_{n-1} - I_{n-1})}{l_{n-1} - I_{n-1}}$$

Through E draw $\overline{h\,q}$, cutting $\overline{D\,d}$ produced at q, then

$$\overline{D\,q} = \overline{F\,h}\,\frac{i_n}{I_n - i_n} = p\,\frac{(I_n + l_{n-1} - I_{n-1})\,i_n}{(l_{n-1} - I_{n-1})\,(I_n - i_n)}$$

Through B draw $\overline{a\,q}$, cutting the vertical through A at a, then

$$\overline{A\,a} = D\,q\,\frac{i_{n-1}}{(l_{n-1} - i_{n-1})} =$$

$$= p\,\frac{(I_n + l_{n-1} - I_{n-1})\,i_n\,i_{n-1}}{(l_{n-1} - I_{n-1})\,(I_n - i_n)\,(l_{n-1} - i_{n-1})} =$$

$$= \frac{M_{n-1}}{H}$$

It will often happen that when $C\,D$ is small that the line $\overline{F\,h}$ is so long that this construction is impracticable and inaccurate. Hence, a different method must be devised. We have

$$\overline{C\,c} = p\,\frac{(I_{n-1} - i_{n-1})}{(l_{n-1} - i_{n-1})}$$

Produce $C\,c$ to intersection with the $\overline{a\,q}$ at c', then

$$\overline{C\,c'} = \overline{D\,q}\,\frac{(I_{n-1} - i_{n-1})}{l_{n-1} - i_{n-1}} =$$

$$= p\,\frac{(I_n + l_{n-1} - I_{n-1})\,i_n\,(I_{n-1} - i_{n-1})}{(l_{n-1} - I_{n-1})\,(I_n - i_n)\,(l_{n-1} - i_{n-1})}$$

The point c' is not known, however, if the construction of h is impracticable, hence, through D draw $\overline{c\;e'}$, cutting $\overline{E\,c}$ produced at e', then

$$\overline{E\,e'} = \frac{\overline{C\,c'}\,i_n}{l_{n-1} - I_{n-1}} = \frac{p\,(I_{n-1} - i_{n-1})\,i_n}{(l_{n-1} - i_{n-1})\,(l_{n-1} - I_{n-1})}$$

Through e' draw the line $\overline{F\;e'}$ and produce it indefinitely to the left, cutting the vertical $\overline{D\,d}$ at K and the vertical $\overline{c\,C}$ produced at some point, say c'' if it be not the same as c', then we will have

$$\overline{D\,K} = \frac{\overline{E\;e'}\;I_n}{I_n - i_n} = p\,\frac{(I_{n-1} - i_{n-1})\,i_n\,I_n}{(l_{n-1} - i_{n-1})\,(l_{n-1} - I_{n-1})\,(I_n - i_n)}$$

$$C\,c' = C\,c'' = \frac{\overline{E\,e'}\,(I_n + l_{n-1} - I_{n-1})}{I_n - i_n} =$$

$$= \frac{p\,(I_{n-1} - i_{n-1})\,i_n\,(I_n + l_{n-1} - I_{n-1})}{(l_{n-1} - i_{n-1})\,(l_{n-1} - I_{n-1})\,(I_n - i_n)}$$

Hence, it is evident that c'' and c' are the same point. Now

$$Z_s = (C_n d_{n+1} - C_{n+1} d_n) l_n = (C_{n-1} d_n - C_n d_{n-1}) l_{n-1}$$

and by inserting the values of the fixed points and reducing, we finally attain

$$\frac{l_n^2}{l_n^2 - 1} = \frac{I_n i_n (I_{n-1} - i_{n-1})}{(I_{n-1} - i_{n-1})(I_{n-1} - I_{n-1})(I_n - i_n)} \qquad (20)$$

whence

$$\overline{D K} = \frac{p l_n^2}{l_{n-1}^2} = \frac{6 E I' h}{l_n^2 - 1} \qquad (21)$$

The application of Eq. (19) is now apparent, and need not be further explained. The quantity $\frac{6 E I h}{l_n^2}$ need only be calculated for a single span, as is evident from Eq. (21).

It might here be remarked that the maximum and minimum strains on a continuous girder are much more easily discussed when the equations are put in terms of the fixed points, and the theoretical results are more easily understood, but this of itself would require a treatise on the subject, which is of itself a large one.

This completes all that is necessary, without going over ground that others have trod, and, as I have shown, all the strains can be obtained *without any analysis whatever* further than is necessary at first to thoroughly comprehend the system. In fact, *one does not even need to know the value of the indeterminate multipliers.* The results obtained are *theoretically accurate*, and their precision is limited only by the scales used and the skill and care of the draughtsman, and as for rapidity, certainly I think no fault will be found on that score, and any one that understands graphical statics sufficiently to construct an equilibrium polygon can obtain all the important strains in a very short time.

It will be observed that all the formulas and diagrams are for unequal spans. This is the most general case, and includes the cases for spans equal, ends fixed, etc., as special cases.

CONSTANT FOCUS AND DEPTH OF FOCUS IN PHOTOGRAPHIC LENSES.

By William A. Cheyney.

Since the introduction of detective cameras we have heard and read much about lenses having a fixed or constant focus for all objects at all distances from the lens, from four feet upward, and this has led to claims being made, for the lenses of different manufacturers, which have been surprising and at variance with all known laws of optics.

This condition of affairs induced me to make a series of experiments, with a number of lenses made by different manufacturers, in order to convince myself as to what extent these statements could be relied upon. The result of which I give you, as follows:

There is no such thing as a constant focus for any lens; the plane of absolute sharpness varies for every point, at which the object may be, between an infinitely distant point and the lens.

That there is a plane of absolute sharpness, in all well-corrected lenses, there can be no question, and the determination of this plane only depends upon the extent to which the image on the ground glass is magnified.

There is, however, a distance through which the ground glass may be moved, and yet the error in the sharpness of the image cannot be detected by the human eye, this distance varies inversely as the ratio of the diameter of the aperture of the lens (the diameter of the opening in the diaphragm used) to the equivalent focus of the lens (the distance from the optical centre of the lens to the ground glass, when an object infinitely distant would give an absolutely sharp image).

This grows out of the fact that the human eye cannot detect an error in sharpness, when the error is not greater than $\frac{1}{200}$ of an inch.

To illustrate this, I present *Fig. 1*.

Here we have the cone of light from a lens working with an aperture of *f*/8 represented by the lines *b d* and *b e* and the position of the plane of absolute sharpness by the line *a b c* (being the position of the ground glass). Now when the ground glass is pushed up to the position indicated by the line *f g h i* we have what would be a mathematical point at *b*, increased to a blurred spot having a diameter represented by the line *g h*, and whenever the length of this line

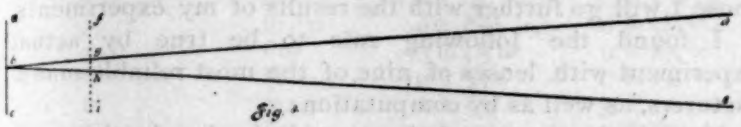

Fig. 1.

g h is $\frac{1}{250}$ of an inch or less, it will be impossible for the unaided human eye to detect the error.

But I stated before that this movement of the ground glass varies inversely as the ratio of the diameter of the aperture of the lens to the equivalent focus thereof, and in order to show this, I will call your attention to *Fig. 2*, given below.

In this figure we have the cone of light from a lens working with an aperture of *f*/16 and the conditions are the same as in the former example, excepting that, while the

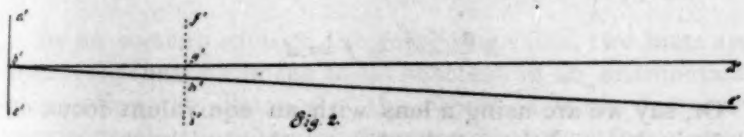

Fig. 2.

line *g' h'* is equal to the line *g h* it will be twice as far from the point *b'* as *g h* was from the point *b*.

Now these conditions are true for a point back of the ground glass.

Here let me say, that this distance from the point in front of the ground glass to the one behind it, between which the error in sharpness is inappreciable, gives rise to what is known as depth of focus in a lens, which is here shown to vary according to the ratio between the diameter of the aperture and the equivalent focus of the

lens and which depends for its existence upon the inability of the human eye to detect the error.

This accounts for the fact that our beautiful little negative, in which we have taken advantage of all the depth of focus of our lens, when it comes to be enlarged gives an unsatisfactory picture. Why? Because we have enlarged the error of focus until the eye is able to detect it.

Still there are some of us who do not desire to make enlargements from our negatives and for the benefit of those I will go further with the results of my experiments.

I found the following rule to be true by actual experiment with lenses of nine of the most reliable manufacturers, as well as by computation:

Multiply the diameter of the aperture of a lens by the equivalent focus thereof; divide the product by the greatest imperceptible error, and to the quotient add the equivalent focus. The sum will be the distance of an objeet upon which a lens should be accurately focused in order that all objects beyond a point one-half of the above distance shall be apparently in focus.

Thus let f equal the equivalent focus, a equal the diameter of aperture and e equal the greatest allowable error; then d will equal the distance of an object upon which, if the lens be accurately focused, all objects beyond $d/2$ will apparently be in focus:

$$\frac{a \times f}{e} + f = d$$

Or, say we are using a lens with an equivalent focus of eight inches and the $f/8$ diaphragm, then we have

$$\frac{1 \times 8}{\frac{1}{250}} + 8 = 2{,}008 \text{ inches} = 167 \text{ feet } 4 \text{ inches.}$$

Now, if we focus upon an object 167 feet 4 inches from the lens, all objects beyond 83 feet 8 inches will be in apparent focus.

Or again, if we use a lens, the equivalent focus of which is four inches and use the $f/16$ diaphragm, we have

$$\frac{\frac{1}{4} \times 4}{\frac{1}{250}} + 4 = 254 \text{ inches} = 21 \text{ feet } 2 \text{ inches.}$$

And here, if we focus on an object 21 feet 2 inches from the lens, then all objects beyond 10 feet 7 inches will apparently be in focus.

As the result of the foregoing, I give the following table:

Equivalent Focus in Inches.	*Diaphragm.*	*Distance of Object Focused Upon.*	*All Objects in Focus Beyond.*
8	f/8	167 ft. 4 in.	83 ft. 8 in.
8	f/11·31	125 ft. 8 in.	62 ft. 10 in.
8	f/16	84 ft.	42 ft.
7	f/8	126 ft. 1¼ in.	63 ft. 0⅜ in.
7	f/11·31	94 ft. 8 $\frac{11}{16}$ in.	47 ft. 4$\frac{11}{32}$ in.
7	f/16	63 ft. 4⅜ in.	31 ft. 8$\frac{1}{16}$ in.
6	f/8	94 ft. 3 in.	47 ft. 1½ in.
6	f/11·31	70 ft. 9¾ in.	35 ft. 4⅞ in.
6	f/16	47 ft. 4½ in.	23 ft. 8¼ in.
5	f/8	65 ft. 6¼ in.	32 ft .9⅛ in.
5	f/11·31	49 ft. 3$\frac{1}{16}$ in.	24 ft. 7$\frac{17}{32}$ in.
5	f/16	32 ft. 11⅝ in.	16 ft. 5$\frac{13}{16}$ in.
4	f/8	42 ft.	21 ft.
4	f/11·31	31 ft. 3 in.	15 ft. 7½ in.
4	f/16	21 ft. 2 in.	10 ft. 7 in.
3	f/8	23 ft. 8¼ in.	11 ft. 10⅛ in.
3	f/11·31	17 ft. 10$\frac{1}{16}$ in.	8 ft. 11$\frac{1}{32}$ in.
3	f/16	11 ft. 11⅜ in.	5 ft. 11$\frac{13}{16}$ in.
2	f/8	10 ft. 7 in.	5 ft. 3½ in.
2	f/11·31	7 ft. 11¾ in.	3 ft. 11⅞ in.
2	f/16	5 ft. 4½ in.	2 ft. 8¼ in.

By an examination of the foregoing table, two facts are seen: (1) That, while the focus shortens in an arithmetical progression, the distance of the object in focus decreases geometrically, thus showing the reason that short focus lenses have a greater depth of focus than those of long focus; and (2) That the distance to the object in focus decreases directly as the diameter of the diaphragm is smaller, thus demonstrating the cause of the increase of depth of focus by the use of smaller diaphragms.

And, finally, you should know that the lenses used in these experiments were of the kinds known as rectilinear or moderate angle, wide angle and landscape, or single combination lenses.

Cheyney, Pa., October 10, 1889.

THE HAIL-STORM AT PHILADELPHIA, OCTOBER 1, 1889.

BY PROF. EDWIN J. HOUSTON.

During the hail-storm, which occurred in Philadelphia on the 1st of October, 1889, some phenomena were presented of sufficient interest to place on record. The storm occurred towards the close of the day, shortly before sunset, and was heralded by the usual banks of dark clouds and strong wind currents in the upper atmospheric regions. It began with a fall of rain, with strong wind, and was followed by a copious fall of hail-stones, varying from small granules, to spheroids of an inch to an inch and a quarter in diameter.

An examination made by the author of some hail-stones that fell on a grass-plot in the northern section of the city, showed the following characteristics, viz:

(1) A cross-section of some twenty or thirty stones showed the usual alternate concentric layers of opaque and transparent ice.

(2) The nucleus of all the hail-stones examined was of opaque ice.

(3) Many of the larger stones had a nearly spherical shape, and were only slightly oblately-spheroidal.

(4) In some of the stones, however, mainly in the smaller ones, the oblately-spheroidal form was so distinctly marked, that the form of discs or flat cylinders was closely simulated.

(5) Nearly all the stones showed an opaque outer layer.

The above points are common to most hail-stones, and are only referred to in connection with a sixth characteristic, which I have never before noticed in hail-stones, nor have I ever known the same to be mentioned in the literature of the subject.

On some of the stones, though not on the majority of them, well marked crystals of clear transparent ice projected

from their outer surfaces for distances ranging from an eighth to a quarter of an inch.

These crystals, as well as I could observe from the evanescent nature of the material, were hexagonal prisms with clearly cut terminal facets. They resembled the projecting crystals that form so common a lining in geodic masses, in which they have formed by gradual crystalization from the mother-liquor. They differed, however, of course, in being on the outer surface of the spherules.

What conditions could have existed in the dense masses of vapor from which the stones received their successive coatings, which permitted the vapor to act as a mother-liquor, despite the violent motions of the stones, so generally assumed to exist during their formation, is difficult to conceive. The formation of such crystals would seem to require comparative rest of the stones in a dense vapor a short period before their final fall to the earth. Is it probable that the crystal-encrusted hail-stones were in a condition of such comparative rest for a sufficient length of time immediately before their fall to the earth to permit the gradual deposition of the well-marked crystals observed?

I know of no theory for the formation of hail-stones, which will permit of such a condition, except the theory of an ascending current, suggested by Mr. Wm. P. Tatham, in an article entitled "A Contribution to Meteorology," which appeared in this JOURNAL (see J. F. I., June, 1889, p. 437, *et seq.*), and it is not improbable, in my opinion, that such air currents were instrumental in producing these peculiar hailstones.

Bearing in mind the low temperature required for the formation of hail, the statements of Oefers, *Akad. Forhandl.*, 1860, p. 439, that under the strong cold attending a fall of snow, ice is formed in the shape of well-formed six-sided prisms, are in accordance with the shape of the crystals observed.

I have referred to the phenomena of the crystal-studded hail-stone, not for the purpose of urging the acceptance of any particular theory for the formation of hail, nor for the purpose of offering any explanation, but for simply placing on record an observed fact.

Unfortunately, I had no camera at hand, and therefore obtained no photographic details of the crystal-studded hail-stones.

CENTRAL HIGH SCHOOL,
PHILADELPHIA, October 12, 1889.

THE SANITARY DISPOSITION OF THE DEAD.

By DR. C. A. HARVEY, New York City.

[*Being the Substance of an Address delivered at the Stated Meeting, held Wednesday, September 18, 1889.*]

It may be safe to say that there is no problem that confronts us to-day which is more fraught with vital importance, from a sanitary standpoint, than that which concerns the best method of disposing of the dead. But it is a question around which gather great and important interests, and not a few difficulties.

There are certain habits and customs of a people which indicate the measure of civilization, of refined sentiment and sense of moral obligation to which they have attained. In nothing, perhaps, do these customs, either in individuals or in a people, more strikingly appear than in the treatment and disposition of the dead. Where these qualities dominate there is manifest for the dead the most profound respect and tender care. Hence, to approach, at this advanced stage of our civilization, a discussion of the subject of "the best means of disposing of the dead" will be to direct attention to a subject for which the way is already measurably prepared. And the more so, because of the considerable attention which is now being given to this subject in nearly every part of the civilized world. The fact that this subject is receiving such attention argues a dissatisfaction among thinking men, with existing and long-practiced methods. A dissatisfaction which is plainly traceable to the light which scientific investigation has

thrown upon the serious evils attendant upon the practice of inhumation. Yet, this method has prevailed so long and is such a time-honored custom, that comparatively few persons ever give a thought to any disposition of human remains other than that of burial in the ground; and few suspect that a method vastly more in accord with the feelings of all who meditate upon the subject, far more congenial to humane sensibilities, and, at the same time, perfectly sanitary, so that the dead shall not perpetually endanger the health and lives of the living, can at once be made available and placed within the reach of all. It is, undeniably, one of the first duties of those who are interested in the public weal—the best interests of their fellows, to seek for and strive to secure to them those conditions which shall be most conducive to the best physical conditions of individuals and communities. Says Sir Spencer Wells: "A knowledge of sanitary science, of the conditions which are necessary for the health of mankind, is still confined to the comparatively few, who may be called the well-educated classes." And he adds that it is "the best educated classes who are most earnest in their efforts to disseminate the branch of knowledge or science which, in the words of Parkes, aims at rendering 'youth most perfect, decay less rapid, life more vigorous and death more remote.'"

It is a marked characteristic of this age, more than of any other, that greater attention has been given to sanitation and to the laws which are conducive to health and longevity.

It is now very widely admitted that one of the most unsanitary customs which has ever been practiced is that of placing dead human bodies in the ground, there to gradually decay and poison the air, the earth and the water, the three elements upon which the living subsist. Indeed, sanitary science has long proclaimed against the time-honored custom of committing the dead to mother earth and leaving to unaided nature the process of resolving the complex compounds constituting the human body to their simple elements; which processes of organic decay are dangerous in the highest degree to the health and lives of the living.

Says Sir Henry Thompson: "No dead body is ever placed in the soil without polluting the earth, the air and the water above and around it." But what would he and other scientists say of the placing of three or four putrifying bodies in the same grave, covered with but two to four feet of soil, and thousands of such graves upon a few acres of ground?

Many of the old burying-grounds of Europe have received so great a number of human remains as to raise the top-soil from one to four feet above the surrounding ground. Many of such burial places have been the fruitful source of epidemics and devastating plagues which have wellnigh depopulated the regions about them. Notable among these was the Cemetery of the Innocents, at Paris, the ground of which had been so filled with buried dead as to raise it some three feet above its normal level. The result was the breaking out of a terribly devastating plague.

Shakespeare seemed inspired to see this danger, as others did not in his day, when he said:

> "The very witching time of night
> When graveyards yawn and hell itself breaks out
> Contagion on this world."

Bascom relates that when the parish church in Winchinhampton, England, was rebuilt in 1843, the earth was removed to change the grade, and the superfluous black earth of the cemetery surrounding it was disposed of for manure and spread upon the adjoining fields. The result was the breaking out of an epidemic of measles, scarlet fever, and various malignant skin eruptions, and the population was nearly decimated. Many similar cases are facts in local histories.

The old English burying law required that but 135 bodies should be buried in an acre, and only one body in a grave. But in practice that number has been exceeded ten times over, indeed, almost indefinitely. In many of the cemeteries of this country, the placing of three or four bodies in one grave, opening it again for each new-comer, is coming to be no unusual thing; warning us that some of our cemeteries

are even now well on the way to those conditions which have caused such wide-spread horrors in other lands. Even beautiful Greenwood, now holding well on towards 300,000 putrifying human remains, is said to be guilty of this reprehensible and outrageous practice.

Recent scientific discoveries have proved that the germs of many infectious and contagious diseases retain their vitality and the power to spread the malady, in the grave and in the earth surrounding it. Yellow fever, cholera, small-pox, splenic fever, scarlet fever, diphtheria, and other contagious diseases can be thus communicated many years after the burial of the dead. Pasteur's researches on the etiology of charbon shows that anthracoid germs are brought to the surface by earthworms; that the earth mould over graves contains the specific germs which propagate the disease, and the same specific germs are found in the intestines of the worms.

Dr. Robert Koch, of the Imperial Sanitary Bureau, at Berlin, detected *bacillus tuberculosis*, and has no doubt but that consumption can be spread by the upturning of the soil of a grave containing the victim of tuberculosis. He also discovered the *comma bacillus* of cholera; and expressed the belief in its propagation in the grave, especially if it be moist.

The outbreak of cholera at Modena, Italy, in 1828, was shown by Professor Bianchi to be due to the upturning of the ground of burial grounds in which victims of the plague had been buried 300 years before.

Dr. Freire examined some soil from a cemetery in Brazil where victims of yellow fever had been interred. Some of this earth was dried and placed in a cage containing a guinea pig. The animal became ill and died within five days. Upon dissection all the tissues presented the characteristic changes which yellow fever brings about, and the brain and intestines were stained yellow by the infiltration of the coloring matter of the cryptococci.

While the *soil* is infested with bacteria or specific disease germs, mephitic gases, also, of the most poisonous character are passing through the soil and escaping into the atmos-

phere. Experiments prove that these gases will rise to the surface through eight or ten feet of earth, and that there is practically no limit to their power of escape. While ammonia and offensive putrid vapors are all given off from bodies decomposing in graves, carbonic acid, which makes cemetery gases so dangerous, is the largest product.

Dr. Playfair affirms that "the importation of graveyard gases entering the blood produces fever; communicated to the viscera it gives origin to diarrhœa, and may be the cause of consumption."

Already the pernicious effects of the 2,000 acres of cemetery grounds in and around Brooklyn are very manifest. The westerly winds sweep those plague spots of corruption and bear their poisonous gases and germs of typhoid fever and diphtheria to the city of Flatbush, making that the most unhealthy community contiguous to the great metropolis, and swelling the death rate to its present alarming proportions. This is only what exists under like conditions elsewhere. According to the report of the French Academy of Medicine, the "putrid emanations of Pere la Chaise, Montmartre and Montparnasse have caused frightful diseases of the throat and lungs, to which numbers of both sexes fall victims every year." "Thus a dreadful throat disease, which baffles the skill of our most experienced medical men, and which carries off its victims in a few hours, is traced to the absorption of vitiated air into the windpipe; and has been observed to rage in those quarters situated nearest to cemeteries."

In a report read before the American Medical Association by Dr. J. M. Keller, of Arkansas, in 1886, he says: "We believe that the horrid practice of earth burial has done more to propagate disease and death, and to spread desolation and pestilence over the human race than does all man's ingenuity and ignorance in every other custom or habit." He adds:

"The graveyard must be abandoned. The time has come for us to face squarely the problem how to dispose of our dead with safety to the living. The earth was made for the living, not for the dead. Pure air, pure water and pure soil

are absolutely necessary for perfect health. Only skeptics deny that the dead do poison these three essentials of human life."

Time will not now permit me to speak of the pollution of underground watercourses by the percolation through graves in which are the oozings from coffins of decomposing bodies, thus infecting wells and streams; or of the pollution by cemetary sepage into water-sheds from which large cities draw their supplies, as the Croton water-shed, in which there are said to be eighty-three cemeteries, large and small, rendering the water wholly unfit for any domestic purposes whatever. That a great sanitary reform in the disposition of the dead is a most important and urgent necessity is too evident to admit of question. Then the practical query which follows this concession is, "what shall be the *nature* of that reform? and *how* shall it be secured?" Its *nature* must be perfectly sanitary, and so appointed and employed that no contamination can occur to either air, earth or water.

Now, I am aware that a considerable number are ready with the suggestion that "Cremation is such a method." But let us look at it candidly and apart from any prejudice we may have for one particular hobby. It is the opinion of eminent jurists that should cremation threaten to become a prevalent method of disposing of the dead, inhibitory statutes would need to be enacted because of its destruction of evidences of crime, either by poisons, malpractice, assaults, or violence of any kind.

Again, suppose cremation were adopted as the prevalent method and crematories were provided with capacity equal to the burning of all the dead, New York, with its suburbs, has about 70,000 dead bodies to dispose of annually, nearly 230 bodies daily, which, if the bodies average 100 pounds each, is equal to 7,000,000 pounds of "green" human flesh to be disposed of annually, or some 30,000 pounds daily. Burn those bodies and the air will be filled with stenches so intolerable that the community would not be long in voting cremation an intolerable and unendurable nuisance, and anything but a sanitary method of disposing of the dead.

When Abraham buried Sarah in the field of Macphelah the question of sanitation was not raised. But when, in 1843, Chadwick and his associates began in London their investigations and exposures of the very unsanitary manner in which the dead of that great metropolis were disposed of, the question of sanitation was raised in so emphatic a manner as to compel public attention and secure the enactment of statutes which provided for vastly better sanitary regulations. So, when at Washington, Pa., at Fresh Pond, L. I., or anywhere else, but three or four bodies are burned in a month, the question of the sanitary effects upon the atmosphere may be scarcely thought of. But the sanitary problem would be greatly changed were all the dead of great populous communities to be burned. Here is a practical difficulty which we have never seen discussed, and for which we know of no absolutely practical solution. That cremation, to a limited extent and as far as it can be purely sanitary, is infinitely to be preferred to inhumation from a sanitary point of view, no one can deny. But there are practical obstacles, both legal and sanitary, which are very serious. And there are, also, obstacles to cremation of a sentimental character which are little if any less serious. A wise Creator and benevolent Father seems to have implanted certain sentimental instincts in the human breast in regard to a fitting deference and respect for departed loved ones, which demands to be recognized and duly respected and which cannot suffer violence without being shocked and pained. So that, however the burning of human remains may be approved as simply a sanitary method, there are manifest reasons which show that it cannot become the prevalent method of disposing of the dead. This brings me to speak of a method against which lie none of the obstacles or objections which are insuperable to the other methods. I refer to a process of *desiccation* of the dead in a finely appointed mausoleum building provided for that purpose. It is a process which is faultlessly sanitary; and, therefore, meets all the requirements in that respect that the most enthusiastic advocate for burning can demand. It provides, as no other disposition of the dead has ever provided, for

meeting and fully gratifying that tender loving sentiment respecting the treatment and disposition of the dead which is so universal.

The sanitary advantages and perfections of this method are secured by the application of advanced science in the use of appliances and in the manner of construction. Magnificent mausoleum buildings, much more grand and elegant than any the world has ever before seen, are provided with a large number of sepulchres which are formed in concrete and arranged in tiers and rows, not wholly unlike the arrangement of the spaces in a safe deposit bank. The sepulchres have one opening, which fronts a corridor, for admitting the body; and, when that is placed within, a plate glass front is hermetically sealed into that opening and this is again covered with a marble or metallic shutter or door and made secure.

There are conduits formed in the concrete, which bring dry air into the sepulchres at one end, and others which take it out at the opposite end. The air, as it passes out, is no longer dry, but is laden with gases and moisture which has absorbed from the bodies, and is now borne through conduits to a furnace, located in an annex, where it passes through the fire and is purified; so that no deleterious gases or offensive odors can ever escape into the atmosphere. The air which is drawn into and passes through the sepulchres is first rendered anhydrous in a large drying room, into which it is forced and from which it is distributed to the sepulchres, where it absorbs the moisture from the bodies in its passage.

By this process a steady current of dry air is pouring into and through the sepulchre, and doing its work most efficiently on its way. The greedy avidity with which dry air seizes and absorbs moisture is known and realized by very few. When a moderate current of dry air envelopes a human body in an air-tight sepulchre, constantly drawing the moisture out of the body and bearing it away, the dry air flowing in as the moisture-charged air and gases are drawn out, the process of desiccation goes steadily on until it is finally accomplished, in from two to five months.

After the work of desiccation is finished the air conduits are closed. As dry air only can be in the sepulchre when it is closed, and as the sepulchre is hermetically sealed and, therefore, atmospheric air cannot reach the now dessicated body, oxidation cannot be expected to ensue. There the body will repose in security and sweetness until the sounding of the resurrection trump.

The fact that by this process the moisture and gases extracted from the body are borne to the fire and cremated, and that, as the process is slow the deleterious gases and vapors pass to the furnace in small quantities at a time and are consumed with the utmost ease and safety, it is thus made the best *sanitary* disposition of the dead.

Again, it is claimed indisputably, that bacteria, microbes and disease germs of every name, live, thrive and propagate in connection with moisture; and that, deprived of moisture and subjected to dry air conditions only, they cease to exist or are entirely innocuous.

Dr. Sternberg, general director of the Hoagland Laboratory, says: "The cholera *spirillum* is destroyed in a few hours by desiccation. The typhoid *bacillus* takes a longer time; but, exposure in a dry condition to oxygen is one of the most effective ways of destroying this and other pathogenic germs." So that, if all germs of disease are not entirely borne away with the moisture and gases and consumed in the flames, and, should any remain, they are destroyed quite as effectually by the dry air in the sepulchre, which is hermetically sealed. Thus, again, rendering this a thoroughly sanitary method of disposing of the dead.

Permit me to say in conclusion, that the plan for the desiccation of human bodies in a New Mausolem, provides for great economy of space in disposing of the dead, rendering one acre of ground equal to thirty-five acres for cemetery use; provides for the most respectful and kindly care for the dead for all time; for perfect security and protection for the bodies placed therein; for guarding against entombment alive; preserves the remains for future removal or for medico-legal examination; provides a home which is equal in elegance to any princely mansion of earth and vastly more enduring, in which the dead may repose without

being disturbed; and, in addition to all these other advantages, its sanitary provisions are perfect, lacking nothing. Thus making it the best method for the sanitary disposition of the dead.

RAMIE.

By Jules Juvenet.

[*A paper read at the Stated Meeting, held October 16, 1889.*]

Dr. Isaac Norris in the Chair.

Mr. Juvenet—Mr. President, Ladies and Gentlemen:

I am here to contribute my share of labor to the success of the ramie industry, which is destined to revolutionize, ere long, the textile markets of Great Britain and America, and moreover to considerably develop the production of the Southern States, as well as the manufactures of the Northern States; but, before entering upon my subject, let me request your indulgence for attempting to address you in a language not my own.

What is ramie?

It was formerly placed by the botanists in the class of *Urtica*, but it is now called *Boehmeria*, or spearless nettle.

I will call it by no scientific name, I will simply name it the richest of all plants, for it possesses wealth of growth, wealth of development and wealth of fibre. In ordinary light ground, with a little watering now and then by rain or irrigation, no plant will grow so rapidly, no root will multiply more quickly and produce more stalks, no vegetable fibre is handsomer, richer or more silky than ramie.

It is a perennial plant, and when once put in the ground, it grows for over twenty years, without replanting; giving, according to climate, two and three crops a year; it is easy of cultivation, requiring only a soil clean and loose; it is planted in straight rows, three feet apart, in a small up-hill form; the plants must be kept very close, in order to shoot

forth straight stalks, without any branches; it grows about like willow, an average of fifteen to twenty switches, from six to eight and ten feet high, covered on the upper part with large green leaves, white underneath. Through its leaves ramie takes its nourishment from the ozone of the air. This developed part of nourishment of the plant, added to the large extensive propensity of the mother-root, from which run horizontally and down a lot of rhizomes and small roots, explain the extraordinary vitality of the plant, and its three and four crops a year in some countries. The Chinese alone have, for a thousand years past, extensively cultivated the ramie plant; before them, the Egyptians were shrouding their dead in magnificent winding sheets of ramie, which to this day are found in the bandages of their mummies. As a textile, therefore, ramie is not precisely a new thing.

How comes the industrial world of this industrial century to be thus backward in introducing practically a plant capable of yielding such important returns and which was made known in Europe by Professor Roseburgh, Director of the Botanical Garden of Calcutta, as long ago as 1803?

The reason is that machinery has been required to do the handwork of the Chinese. No machine can do it at once practically; it requires machinery good for planters, enabling mere separation of the crude bark of the stalks, leaving to industry and chemical agents the task of eliminating the gummy and resinous matters encasing the fibre.

It is then not only a practical machine to peel the bark from the ramie stalks that we want, but also a cheap chemical process to dissolve the foreign matters around the white textile, which has also to be spun on special machinery. Only the perfect knowledge of these connected operations, managed in a business way, will assure in this country the success of the ramie industry.

In a rapid manner I have already explained above the mode of culture of the plant, and I am now preparing to be distributed over all the Southern States a pamphlet giving detailed information on the different phases of the ramie culture.

There is no doubt that Southern planters will go extensively into said culture, when we have here in Philadelphia mills to turn their raw products of ramie into thread finer and stronger than the finest flax thread.

After the cultivation of ramie we have to know how to harvest it, or decorticate it, then to bleach the same and spin it.

It is these three operations that I want to explain here, and on which we experimented last week at Bloomsdale Farm, near Bristol, Pa.

A five-acre field was planted with ramie there in May of this year, for experimentation only, because, in a general way, the ramie culture is not well adapted to this section, the frost here killing the root during the winter; but Philadelphia is an enterprising textile place, and wants to have at hand every element of success. To illustrate what has been done at Bloomsdale, I bring here some green ramie stalks from that place.

A minimum of fifteen of these are growing in a bunch on each plant, and there are 10,000 plants in one acre, say from 150,000 to 200,000 stalks to the acre; an efficient machine had then to be devised to harvest rapidly such a number of stalks.

The decortication or peeling of the fibre from the stalk is done in the following way: (1) By beaters to strip the leaves; (2) A crusher to break the woody part; and (3) Other beaters to knock out the wood and get the bark in the way I show you here, which, henceforth, will be the marketable article for planters of ramie. Then the chemical treatment will come in and dissolve the gummy matters in order to get the white textile, which will be spun in Philadelphia. The crude bark is chemically composed as follows:

The cellular portion, embracing cellulose, paracellulose, metacellulose; then vasculose, pectose, cutose, albuminous substances, pectate of lime, some mineral matters in very small quantities.

The cellulose, of which there is about seventy per cent. in Louisiana ramie, is the fibre itself. We have to keep it intact, to get our pure ramie textile, henceforth we have,

then, to eliminate the vasculose, pectose and cutose more or less according to the purposes for which the material is wanted, or for cordage, lace, or damask. If it is for cordage, cutose only must be dissolved, and if for lace or damask, vasculose, pectose and cutose must be dissolved to leave the cellulose or fibre entirely pure.

As a guide, it should be noted, that boiling diluted hydrochloric acid dissolves pectate of lime, setting free pectic acid, which may be neutralized by an alkali, and it also transforms pectose into pectine, which can be precipitated by alcohol.

Cellulose is dissolved by cupro-ammonium solution; and boiling hydrochloric acid renders paracellulose soluble in cupro-ammonium. Bi-hydrated sulphuric acid dissolves cellulose substances. A boiling solution of potassa dissolves cutose, and under pressure it dissolves vasculose. Diluted nitric acid renders vasculose soluble in alkaline solutions.

The bleaching process consists then in applying the chemical agents in suitable proportions to dissolve slowly the foreign matters we want to get rid of, which are then washed away in running water. The bleaching being done, the fibre is then ready for the mill—but no spinning mill for ramie exists as yet in the United States, and the cotton, wool, flax and hemp spinners cannot put ramie on their machinery. It requires from the softener to the spinning-frame, provided with numerous small spindles, a series of from eight to ten machines well connected together. When the ramie has passed through these machines it is then turned into strong, fine, glossy thread, from No. 10 to No. 80, which any weaver of the land may use with advantage.

The problem of the practical utilization of ramie, is at last on the very road to success in all its branches; this has not been obtained without study and loss of money and time.

To agriculturists, I will say, that I have often weighed ramie stalks, grown by myself, in Louisiana; I used to take 150 stalks fully grown, about six feet high; those 150 stalks representing the minimum crop of ten plants. It gave me the following figures:

Plants.	*Stalks.*	*Weight of Green Stalks with Leaves.*	*Weight of Green Stalks without Leaves.*	*Crude Fibre, Wet.*	*Crude Fibre, Dry.*	*Bleached Fibre.*
		lbs.	*lbs.*	*lbs.*	*lbs.*	*oz.*
10	150	49	29	6	1	7½

There are 10,000 plants in one acre, or 1,000 times more than the above table, say 1,000 pounds of crude bark or fibre, dry, and 437 pounds of bleached ramie, per crop, and as there will be two crops if not three yearly, the gross return of an acre of ramie can be easily calculated when I add that crude bark of ramie, dried and baled, is worth from three to five cents a pound, according to quality. I will say, also, that there is in the South not much danger of bad crops of ramie, because it is easy of cultivation, and there is no fear of frost, or of cotton worm, on account of the tannin contained in the plant, on which account, insects rarely attack ramie plants.

This new product of the Southern States being brought to Philadelphia to be bleached and spun, will advance the welfare of this city as has the carpet trade.

Philadelphia is now the greatest carpet-making city in the world. Only about thirty years ago the industry here had its beginning with John Bromley, who worked two looms in a small building. There are now 172 establishments, occupying over 200 large structures, working 7,350 looms and employing 17,800 workmen. They produced last year 71,500,000 yards of carpet, worth nearly $48,000,000. And the price to the consumer has been reduced about one-half.

When ramie shall be woven alone, or with cotton, wool, flax or silk, as samples here, by every mill in the country, the public at large will find:

That ramie has twice as great strength as flax and hemp, that it washes much better than any other textile and becomes whiter than hemp and flax.

That ramie, when properly worked, has the lustre of silk, to such an extent that it is used for many fancy articles, dresses, fine passementeries, portières, plush, etc.

That ramie is more hygienic than flax, hemp or cotton, and that its use is recommended in several hospitals for dressing wounds.

That ramie does not rot in water, and that on this account it is in great demand in the Navy for sails, cordage, fishing tackle, and wherever the quality of resistance to the atmosphere and water is needed.

With all the elements of success above explained, and even in the incessant and daily progress which this century makes in all things, commerce, industry, literature, art, science, a new industry of any scope and importance necessitates for rapid development a large centre of production and consumption. Philadelphia, I hope, will be this centre for the ramie production of the South, and the ramie manufacture and consumption for the whole of this great Republic.

REVIEW OF THEORIES OF ELECTRICAL ACTION.*

By Professor H. S. Carhart.

The Physics Section of this Association congratulates itself because it deals with topics of the most lively and general interest, not only from a practical point of view, but still more from a theoretical one. Even popular interest in electricity is now wellnigh universal. Its applications increase with such prodigious rapidity that only experts can keep pace with them. At the same time the developments in pure electrical theory are such as to astound the intelligent layman and to inflame the imagination of the most profound philosopher.

Of the practical applications of electricity it is not necessary to speak. They bear witness of themselves. A million electric lamps nightly make more splendid the lustrous name of Faraday; a million messages daily, over land and under sea, serve to emphasize the value of Joseph Henry's contribution to modern civilization. Blot out these two names alone from the galaxy of stars that shine in the

* Address by Professor Carhart, Vice-President Section B, American Association for the Advancement of Science, delivered at the annual meeting, Toronto, August 28, 1889. Abstract from *The Electrical Engineer*.

physical firmament, take from the world the benefits of their investigations, and the civilization of the present would become impossible. The value of the purely scientific work of such men is attested by the resulting well-being, comfort and happiness of mankind.

But the mind can never rest satisfied with the facts and applications of a science, however interesting and useful they may be. It feels an inward impulse to link the facts into a related whole, to inquire into their causes, to frame a satisfactory theory of their correlation, and so to build on them a true science.

It is, indeed, interesting to study the history of any scientific doctrine and to trace its development from the crude notions of its earliest stages to the more refined conceptions of later periods, comporting indefinitely better with the marvellous processes of nature. Such a history we have in the views which have been held regarding the nature and action of electricity. The transition from the glutinous effluvium of the sagacious Robert Boyle to the magnetic and electric waves of the present, traversing the omnipresent ether with the velocity of light, is not an easy one to make, even in a period of 200 years.

For more than twenty centuries natural philosophers had nothing better than the emission theory to account for the attraction exhibited by rubbed amber and other similar substances. Their notion was that the rubbing of the amber caused it to emit an effluvium which returned again to its source and carried light bodies back with it.

In one respect this fanciful attempt to explain electrical attraction deserves commendation, for it evinces a mental inaptitude to account for physical actions "at a distance," or without some intermediate agency. Later philosophers, satisfied perhaps too easily with mathematical explanations founded on the observed laws of attraction and repulsion, and not demanding a medium, did not feel the same intellectual necessity of filling the space between bodies acting on one another, either with emanations from those bodies or with an invisible, imponderable medium, suspected by no sense of man, but required only to meet a demand of his

highest intelligence. For when the Newtonian philosophy had made some progress, the doctrine of unctuous effluvia was given up, and physicists acquiesced in the unexplained principle of attraction and repulsion as properties of certain bodies communicated to them by the Divine Being, the mechanism of which they scarcely attempted to explain.

"Many superficial philosophers thought they had given a very good account of electricity, cohesion and magnetism by calling them particular species of attraction peculiar to certain bodies."*

The discovery by Stephen Grey that "the electric virtue" could be conveyed along a wire for several hundred feet without sensible diminution, and the invention of the Leyden jar by Kleist, or Cuneus, had the effect of annihilating many mushroom theories constructed on the slimmest basis of facts. The latter discovery disclosed a power in electricity not previously suspected, and excited the greatest interest in both Europe and America.

At this period Franklin turned his attention to the subject, and "spent more time in diversifying facts and less in refining upon theory" than some of his European contemporaries. In fact, he tells us that he was never before engaged in any study that so totally engrossed his attention and his time. His discovery that the two electricities are always excited in equal quantities, that the charge resides on the glass and not on the coatings of the Leyden jar, and his experimental identification of lightning with frictional electricity, excited the liveliest interest abroad, and secured for him the Copley medal of the Royal Society; while his theory of positive and negative electricity made a permanent addition to the nomenclature of the science. His conceit that a turkey, killed with the discharge of a battery of jars, was uncommonly tender eating, a discovery gravely communicated to the Royal Society by William Watson, is not so well known, and does not appear up to the present to have been verified.

We cannot agree with him, I am sure, when he says:

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* Priestley's *Hist. of Elec.*, vol. ii, p. 18.

"Nor is it of much importance for us to know the manner in which nature executes her laws; it is enough if we know the laws themselves."

For the pursuit of the manner in which nature executes her laws is the distinguishing characteristic of the science of the present day. It has led to most brilliant discoveries, and bids fair to do more than all other agencies combined to show the intimate and necessary relations existing between the different branches of physics. We need to be reminded often that accumulated facts do not constitute a science; and that utility is not the highest reward of scientific pursuits.

A bit of polished marble, plucked from the ruins of the Roman Palatine Hill, is an interesting relic; but how much more interesting to reconstruct the palace of Nero and to see this fluted marble in its proper and designed relation to the whole, of which it was once a necessary part. Science is constructive. Laws are derived from an attentive consideration of facts; generalizations group laws under broader relationships; and great principles unite all together into one related, impressive whole.

From the time when the famous Boyle caught sight of a faint glimmer of electric light to the present, physicists have been in pursuit of the connection between light and electricity. As early as Newton's time, the ether was conceived by some to be a subtle medium confined to very small distances from the surfaces of bodies, and to be the chief agent in all electrical phenomena. "But," says Priestley,* "the far greater number of philosophers suppose, and with the greatest probability, that there is a fluid, *sui generis*, principally concerned in the business of electricity. They seem, however, though perhaps without reason, entirely to overlook Sir Isaac Newton's ether; or if they do not suppose it to be wholly unconcerned, they allow it only a secondary and subordinate part to act in this drama." Among the branches of knowledge that this writer recommends as likely to be of especial service in the study of

* *Hist. of Elec.* vol. ii, p. 22.

electricity is the doctrine of light and colors. The invention of the voltaic battery, and Sir Humphry Davy's celebrated experiment in producing the electric arc, stimulated inquiry in this same direction. Mrs. Somerville, Morrichini and others sought to produce magnetism by means of sunlight, but ultimately, as is now known, without success. Notwithstanding these negative results, Faraday had such a "strong persuasion derived from philosophical considerations" of a direct relation between light and electricity, that he resumed the inquiry in a most searching manner, with the happy result of discovering the rotation of the plane of polarization of light by means of magnetism.

"Thus is established," he says,* "a true, direct relation and dependence between light and the magnetic and electric forces; and thus a great addition [is] made to the facts and considerations which tend to prove that all natural forces are tied together, and have one common origin."

It was thus reserved for Faraday to make those discoveries and to obtain that insight into electric and magnetic action which were needed by his great disciple and interpreter, Maxwell, to construct a most marvellous theory of the connection between these two departments of physical science.

Respecting the failures to obtain magnetism from the direct action of sunlight, to which allusion has been made, Maxwell says that we should not expect a different result, because the distinction between magnetic north and south is one of direction merely; that there is nothing in magnetism indicating such opposition of properties as is seen at the positive and negative poles of a battery in electrolysis; that even right and left-handed circularly-polarized light cannot be considered the analogue of the two poles of a magnet, for the two polarized rays when combined do not neutralize each other, but produce plane-polarized light.

It may be said, however, that if a right-handed circularly-polarized ray produces magnetism in one direction, and a left-handed ray in the opposite, then the combination of the

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* *Exp. Researches*, 2,221.

two rays may neutralize their magnetic effect, inasmuch as plane-polarized light may have no magnetic influence. Professor J. J. Thomson has lately shown mathematically that a circularly-polarized ray does have a magnetic effect, but that it is so small, even with strong sunlight, as to be much beyond the limits of experiments; and Mr. Shelford Bidwell has produced a bar of iron in such an exquisitely sensitive magnetic state, that magnetic changes are certainly produced in it by the direct action of light. This he has secured by rendering the bar more susceptible to magnetic influences in one direction than the other. We may not, I venture to affirm, be without hope that magnetism and electric currents may yet be evoked by the direct agency of sunlight.

Faraday was deeply convinced that space had magnetic properties, and that the space or medium around a magnet is as essential as the magnet itself, being a part of the complete magnetic system. To him all magnetic and electric action took place by contiguous particles along lines of force. "What that magnetic medium, deprived of all material substance, may be, I cannot tell," he says,* "perhaps the ether." No doubt existed in Faraday's mind that these lines represent a state of tension; but whether that tension is a *static* state in the ether, or whether it is *dynamic*, resembling the lines of flow of a current between the poles of a battery immersed in a conducting fluid, was uncertain. He inclined, however, to the latter view. He was thus lead to advocate, though not without hesitation, the physical nature of lines of force.

Faraday's discoveries and his method of regarding all magnetic and electric actions as propagated through a medium by means of contiguous parts have been of the utmost productiveness. They have revolutionized the science of electricity, and have been the most potent factors in the genesis of a theory, including all radiant energy, which has recently received such remarkable and conclusive confirmation. His name has become almost a household

* *Exp. Researches*, 3,277.

word. His earnest, unselfish life has added unnumbered millions to the world's wealth. His ideas and words, which have been instruments in the hands of philosophers, have become the current coin of the commercial tyro, who talks as glibly about lines of force and the magnetic circuit as if he really knew something about them.

Fruitful as Faraday's ideas were, they yet awaited a mathematical interpreter for their highest development. A good Providence sent James Clerk Maxwell, whose brilliant mathematical ability was equalled by his philosophic insight, his poetic feeling and imagination, his profound sincerity and his great sympathy with nature.

To appreciate Maxwell's relation to theories of electrical action, it is desirable to take a retrospect of the views that have been held regarding its nature. Three periods in the history of these views may readily be distinguished. The first was introduced by Dr. Gilbert in 1600, and it lasted for about 225 years. The little that was known previous to Gilbert constitutes only the preface or introduction to the history proper. Nearly three-fourths of this period was utterly barren and unfruitful. It knew nothing better than unctuous effluvia and electric atmospheres. In the latter half of the period the Newtonian philosophy had become the orthodox doctrine. The great success attending the mathematical investigations, founded upon the law of inverse squares, naturally carried with it the acceptance of the underlying hypothesis of "action at a distance." There were not lacking, indeed, men of deeper philosophic insight who denied this doctrine, which they looked upon as entirely unphilosophical and which must utterly bar the way to any inquiry into the process by which the law is executed. Action at a distance by attraction or repulsion, varying inversely as the square of that distance, means an ultimate fact not admitting of further analysis.

The second period was one of contention. It began, not with the important discovery of current electricity, nor of the electro-magnet, but with the philosophical methods and concepts of Faraday. The physical postulates of the mathematical school were entirely alien to the views which

he adopted. "Faraday, in his mind's eye, saw lines of force traversing all space where the mathematicians saw centres of forces attracting at a distance; Faraday saw a medium where they saw nothing but distance; Faraday sought the seat of the phenomena in real actions going on in the medium, they were satisfied that they had found it in a power of action at a distance impressed on the electric fluids."* Prior to Faraday the supporters of a medium to explain electric and magnetic action were always thrown out of court for lack of evidence; Faraday gave them a legal standing by furnishing the facts and evidence on which they could well afford to base their case.

The corpuscular theory of light, which had shown such remarkable vitality, was now in the last stages of a fatal disease, due to indigestion and lack of assimilation. Foucault finished it off in 1865 with his crucical experiment to decide upon the relative velocity of light in the air and water. The undulatory theory was thus fully established, and the doctrine of radiant energy in general began to be clearly apprehended. The grand generalization of the conservation of energy was looming up all along the horizon of science, as the towers and spires of a great city appear to rise out of the sea to a traveller approaching the land. Victory was ready to perch on the banners of an army contending for the ether doctrine—not a decimated army, but one constantly augmenting in numbers by deserters from the enemy. At this period, sixteen years ago, appeared the epoch-making book of Maxwell on *Electricity and Magnetism*. Its author professes only to translate Faraday's ideas into mathematical language; but he did vastly more than this. He demonstrated mathematically that the properties of the medium required to transmit electro-magnetic action are identical with those of luminiferous ether. It would be unphilosophical, he remarks, to fill all space with a new medium whenever any new phenomenon is to explained; and since two branches of science had independently suggested a medium requiring the same properties to account

* *Maxwell's Elec. and Mag.*, p. x.

for the same phenomena in each, the evidence for the existence of a single medium for both kinds of physical phenomena was thereby greatly strengthened. The step from identity of the medium to identity of phenomena, that is, that light itself is an electro-magnetic phenomenon, though it may now seem to be a short one, must nevertheless, upon careful consideration, always be accepted as evidence of the greatest genius. To walk in Maxwell's footsteps now and take the very steps he took is one thing, and a comparatively easy one; but to make original explorations into unknown regions of nature, and to tread where no human being has ever before set foot, is quite another thing. The electro-magnetic theory of light must be regarded as a great generalization, inferior only to that greatest one of all time—the conservation of energy.

The principal criteria upon which Maxwell relied for the confirmation of his theory may be briefly enumerated:

(1) An electro-magnetic wave or undulation is propagated through the ether with a velocity equal to the ratio of the electro-magnetic to the electro-static unit of quantity. If light is an electro-magnetic phenomenon, its velocity must also be equal to this same ratio. The very close approximation of the one to the other, as determined by a variety of methods, has been known for some time.

(2) The specific inductive capacity, K, of any transparent dielectric should equal the square of its index of refraction. The discrepancies at this point are so great that all one can say in the most favorable case is that K is the most important term in the expression for the refractive index, while in other cases no confirmation whatever can be drawn from this class of evidence.

(3) The magnetic and electric disturbances are both at right angles to the direction of propagation of the wave and at right angles to each other. The mathematical form of the disturbance agrees with that which constitutes light in being transverse to the direction of propagation. Further, the *electric* disturbance should be perpendicular to the plane of polarization of plane-polerized light.

(4) In non-conductors the disturbance should consist of

electric displacements, but in conductors it should give rise both to electric displacements and electric currents by which the undulations are absorbed by the medium. Most transparent bodies, it is true, are good insulators, and all good conductors are opaque. The degree of opacity is, however, far from being proportional to the conductivity.

(5) But perhaps the most important criterion of all is the one relating to the very existence itself of a medium. Such a test lies in the *time* element involved in transmission from point to point. Since energy is transmitted from a luminous body, as the source, to another body, which may absorb it, then plainly, if time is required for the transmission, the energy must reside in the medium by which the transmission is effected during the interval between the emission and the absorption. In the emission theory the light corpuscles are the receptacles of the energy and carry it with them in their flight. According to the undulatory theory, the medium filling all space is the receptacle of the energy, and passes it along from point to point by the action of contiguous parts.

Foucault's *experimentum crucis* proved the emission theory untenable. Roemer's observation of the retardation of the eclipses of Jupiter's satellites, when the earth is moving away from Jupiter, is, therefore, a confirmation of the undulatory theory of light and, in consequence, a demonstration of the existence of the luminiferous ether.

At this point the history of the nature of electrical action touches upon the third period.

The period upon which we have just entered may not inappropriately be called the period of confirmation. Nothing further appears to be necessary for the complete demonstration and establishment of the electro-magnetic theory of light. The noteworthy experiments of Professor Hertz, of Carlsruhe, are known to all. Rightly conceiving that the reality of electro-magnetic waves would be best established by the same experiments which would also establish the fundamental identity of such undulations with those of light, he had recourse to the principle of resonance or sympathetic vibrations for the detection of these long-period

waves. By a device no less remarkable for its simplicity than its effectiveness he produced electrical oscillations of such rapidity that the waves in the surrounding region were short enough to be measured. This he accomplished by attaching to the secondary terminals of an induction coil two rectangular sheets of metal, each supplied with a short, stout wire ending in a small ball. The balls were brought near each other and the discharges of the coil took place between them. Under these conditions the discharge is oscillatory, and the period may be calculated by the formula of Sir Wm. Thomson, published in 1853.*

The receiving apparatus is also of the simplest design, consisting ordinarily of a circle of wire, interrupted at a point with an adjustable opening, and of such dimensions that the waves passing through the circle may set up electrical oscillations in it, synchronizing with those of the transmitting apparatus. The passage of sparks across the narrow opening of the circle indicates an electrical flow; and the necessity of adjusting the size of the circle in order to obtain this flow proves that the forces acting are periodic. The receiving apparatus must in fact be tuned so that the period of an electrical oscillation in it shall correspond with the external impulses absorbed. The intensity of the electric and magnetic disturbances is indicated by the relative length of sparks obtainable.

Equipped with this apparatus, which was installed in a large lecture hall, Hertz found not only that his tuned receiver responded to the impulses of the transmitter in the precise manner pointed out by theory, but that the sparks showed a series of maximum and minimum values recurring in periodic order as the receiver was carried further away from the source of the disturbances. The astounding fact was thus brought out that these electro-magnetic waves were reflected from the thick wall of the room, and that the combination of the direct and reflected systems produced stationary waves with loops and nodes that could be traced out by the responsive circle of wire. In this manner wave

* *Math. and Phys. Papers*, vol. i, page 540.

lengths were measured down to sixty cms., and the *time element* was experimentally detected in the propagation of electro-static and electro-dynamic induction. It was demonstrated that the disturbances producing the waves are at right angles to direction of propagation, as Maxwell predicted, and as interference phenomena show them to be in light. Hertz has also found an electro-dynamic shadow cast by an iron post; he has verified the laws of reflection from plane and concave metallic reflectors, and has shown that electric waves suffer polarization and refraction in a manner exactly analogous to light. Professor Fitzgerald, of Dublin, has added another confirmation of Maxwell's doctrine, demonstrating that the *electric* disturbance is perpendicular to the plane of polarization, as Maxwell's equations require. Finally, the velocity of propagation of these electro-dynamic waves is found to be the same as the velocity of light. Thus not only have all of Maxwell's criteria, except the second, abundantly confirmed the judgment of the great physicist, but other proofs have been added. Electro-magnetic waves are therefore not merely like light, but they are light. Or perhaps, to speak more exactly, all radiant energy is transmitted as electro-magnetic waves in the luminiferous ether. Electricity has thus annexed the entire domain of light and radiant heat; and, as Professor Lodge says, "has become a truly imperial realm." The difference of wave length in the three classes of phenomena is not a fundamental one. Increase the rate of the electrical oscillations a million-fold in Hertz's experiments, and the waves would not merely resemble light—they would be light. A wire through which such oscillations are surging back and forth would glow with light. Even the long heat waves would be absent, and only those producing the sensations of light and color would remain.

It will be observed that the oscillations of an electric discharge constitute the point of departure for the admirable researches of Hertz; and it is a matter in which we may modestly take a bit of national pride that the first case of electric oscillations was discovered by an American physicist. The oscillatory character of the Leyden jar dis-

charge was demonstrated by Joseph Henry, in 1832, by means of the magnetic effects produced in small steel needles. It was not until twenty-one years later that Sir Wm. Thomson published the complete mathematical theory of such oscillations. They have since been observed directly by means of a rotating mirror. Dr. Oliver Lodge has lately shown that they rotate the plane of polarization of light in one direction and then in the other as they surge back and forth. He has also reduced the number of oscillations from several millions per second to a few hundred by increasing the capacity and the self-induction. The discharge then vibrates within the limits of audibility and produces a musical note.

The well-known experiment of Henry, in which he observed an induction current in a wire stretched parallel to and distant thirty feet from one which served to discharge a Leyden jar, is now seen to have been a case of resonance; that is, the absorption of electric waves by a conductor, producing currents therein. And it is an evidence of the great genius of Henry that he saw, somewhat dimly it may be, but still with a certain degree of rational apprehension, that the induction was transmitted across the intervening space with a velocity comparable only to that of light. He had perchance the divine touch of genius necessary for the great discovery of electro-magnetic waves coursing through the ether; but the way leading to this important physical fact had not then been sufficiently prepared, and its discovery was impossible.

Waves, similar to those from a Leyden jar discharge, but of longer period, are sent out from a wire conveying alternating currents. We must conceive of such a wire not simply as affected internally or even superficially by the electric energy surging through it, but as the source from which pulsate outward through the limitless ether great waves of electro-magnetic disturbance. For 300 complete alternations per second, these waves are a million metres, or over 600 miles, in length. They present a marked contrast with the waves corresponding to the D lines of the spectrum, which are only about one five-millionth of a millimetre long.

These long waves from an alternating current represent energy. Through space it is conveyed with the velocity of light, and through other non-conductors or dielectrics with a smaller velocity, precisely as in the case of the radiant energy of light or heat. Henceforth the complete equation for the distribution of energy by means of alternating currents must include a term to express the radiation from the circuit. It may indeed be found that this term represents no inconsiderable part of the energy communicated to the wire in the case of very rapid alternations.

Thus we see that the ether plays a magnificent role in what may be called its dynamic relation to electric displacements. In its capacity as a reservoir of static or potential energy its agency has been better understood for a considerable period. When a continuous current begins to flow through a closed circuit, a single wave travels out from the conductor; and during its progress, while the current is approaching its constant value, the inclosing ether is assuming its condition of static repose under stress. The whole ether, extending indefinitely outward from the conductor, is profoundly modified.

We know how to map out the circular lines of force about it by means of iron filings; but the iron serves only to show what has already taken place in the ether before the filings are brought into the field. Every little iron particle becomes a magnet, with all the north-seeking poles stretching in one direction round the wire, and all the south-seeking poles in the other. What the mechanism of the stress, or the motion in the ether to produce these effects, may be, we do not know; but we do know that these lines of force are all subject to a tension tending to shorten them, and that they are mutually repellent laterally. When a current is sent through a conductor, the ether is expanded in concentric cylindrical layers about any straight portions of the circuit, and becomes the reservoir of potential energy.

As soon as the current, which maintains this state of tension, ceases to flow, the stretched ether collapses upon the conductor, yielding up its energy in the form of self-induction. If a steady current is conceived as the setting-

up and breaking-down of a static difference of potential energy at infinitesimal intervals of time, then the energy transmitted may depend upon a similar formation and decay of the static stress in the encompassing ether. The conductor is but the core of an electro-magnetic disturbance in the surrounding medium; and it may be that the enormous energy which a small copper wire can apparently convey is in reality transmitted by the invisible medium.

From this brief review of the theory of electric action it will be quite evident that henceforth the language applied to electrical phenomena must always include the luminiferous ether as a prominent term. The experiments of Hertz have made it impossible to explain electrical facts without taking this invisible medium into account. There is no such thing as electric or magnetic action at a distance. The ether is always an essential part of that complex system, the interactions of which manifest themselves as electric or magnetic phenomena.

As the ear responds to the slow oscillations of an electric discharge through the intermediate agency of heat, so the eye of the mind responds to those more rapid oscillations, the existence of which has been demonstrated by experiment. No less clearly does the magnetic field appear as a system of lines of stress in the ambient ether. Definiteness has taken the place of the metaphysical speculations of earlier times. Complete ignorance has, at least, been superseded by half knowledge. We may not yet affirm with Edlund that the ether *is* electricity, but we are doubtless nearer a solution of this old problem than ever before.

PROCEEDINGS

OF THE

CHEMICAL SECTION,

OF THE

FRANKLIN INSTITUTE.

[*Stated Meeting, held at the* INSTITUTE, *Tuesday, October 15, 1889.*]

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, October 15, 1889.

MR. T. C. PALMER, Vice-President, in the Chair.

Members present: Dr. L. B. Hall, Dr. Wm. H. Wahl, Prof. N. Wiley Thomas, Mr. W. W. McFarlane, Mr. C. J. Semper, Mr. Lee K. Frankel, Mr. Reuben Haines, Mr. A. Weikel, Mr. J. H. Eastwick, Mr. F. C. Lewin and a number of visitors.

The following gentlemen were nominated for membership in the Section: Mr. Geo. L. Norris, Pencoyd, Pa.; Mr. Hugh A. Galt, 1011 Spruce Street, Philadelphia; Mr. Lucius E. Williams, Swarthmore College, Swarthmore, Pa.; Mr. Hermann Schanche, Gray's Ferry Chemical Works, Philadelphia.

Dr. Wahl made some remarks on the industrial production of aluminium by electrolysis, supplementing his comments of last meeting. He referred to the process of M. Adolphe Minet, in operation industrially at Creil (Oise), France, and which in essential parts was identical with that of Mr. Hall. Mr. Minet electrolyzes a bath composed of forty per cent. of cryolite and sixty per cent. of chloride of sodium, maintained in the state of igneous fusion. The bath is continually regenerated by additions of alumina (or bauxite). The interesting point in considering the two processes, the speaker stated, was the different explanations of the operations taking place in the bath under the action of the electric current upon substantially the same components; Hall claiming that the alumina undergoes electrolysis, the solvent fluorides being unaffected, and Minet that the fluorides are electrolyzed, the liberated fluorine acting upon the alumina, to form fresh fluoride, thus maintaining the bath, the explanation of M. Minet, the speaker held, to be the rational one. Adjourned.

WM. C. DAY, *Secretary.*

NOTES AND COMMENTS.

ENGINEERING.

BRIDGING THE ENGLISH CHANNEL.—One of the papers read before the meeting of the Iron and Steel Institute, at its recent meeting in Paris, was fowarded by M. Schneider, of Creuzot, and M. Hersent, ex-President of the French Civil Engineers' Society, on the Channel Bridge, and gave an elaborate account of the scheme. The route chosen as the line, stretching over the shallowest parts of the Channel and connecting the shores where closest to each other, commences at a point near Cape Gris Nez, passes over the Colbert and Varne banks, and terminates near Folkestone. The Colbart and Varne banks are situated near the centre of the Channel, about six kilometres (3·72 miles) apart, the depth of the water at that point not exceeding seven or eight metres (twenty-three to twenty-six feet) at low water, and are separated from each other by a depression about twenty-five metres to twenty-seven metres deep. Between the Varne and the British coast the depth does not exceed twenty-nine metres, but near Colbart the bottom sinks somewhat abruptly down to forty metres. It then attains fifty-five metres (180 feet) about midway across, when it begins gradually to rise. In these parts the chief difficulties would be encountered in laying the foundations. The result of repeated experiments is that the ground is found to be sufficiently solid to support very extensive works, and the borings lately made in connection with the proposed tunnel have confirmed preceding experiments as to the position and nature of the bottom as published by M. de Gamond. More precise inquiries will be necessary, when the works are proceeded with, as regards each pier, in order to be in a position to solve each detail beforehand. The metal to be used is steel. The extensive use of it made lately, both in France and abroad, notably in the Forth Bridge, removes every doubt as to the feasibility of dispensing with about fifty per cent. in weight by the use of steel, while insuring the same degree of safety. The amount of metal and machinery to be provided would represent an aggregate weight of about 1,000,000 tons, the assumption being that each country will have to supply one-half of this amount. A powerful impulse would, for a long period, be given to national industry. The many improvements made in the art of bridge building warrant every hope of success in an attempt to turn out spans of metal 500 metres (1,640 feet*) in length across the Channel, supported by columns resting at different depths on the bottom of the sea. A rough calculation gives, with reasonable certainty, 380,000,000f. for masonry supports, and 480,000,000f. for the metallic superstructure—in all, 880,000,000f. or £34,400,000 ($170,000,000). The works for the tunnel and the railways of both countries would have to be planned later on, in agreement with the companies whose lines would lead up to the bridge. The time required for the undertaking may be fixed at about ten years. The whole of the pillars

* The spans of the Forth Bridge are 1,700 feet.

will occupy a little over one twelfth of the section of the Channel. This reduction of the section of the Channel is not likely to exercise a notable influence on the erosion of the bottom, or to bring about an appreciable increase of the speed of the flood and ebb tides. The distance between the piers, fixed at 500 and 300 metres for the large spans, will not be less than 200 and 100 metres respectively for the small ones, and will be sufficient to prevent their proving an obstacle to the free navigation of sailing vessels. As regards steamships, no such danger is to be apprehended, as the current, which would become a little faster in the centre of the open spans, would carry floating bodies, even disabled vessels, towards that part, and prevent their ever touching the bridge. Owing to these distances and dimensions, the piers would, in no way, modify the conditions of navigation in the Channel, and would not constitute an appreciable obstacle to navigation in general.

As for the metallic superstructure, the metal columns firmly placed upon the platforms of the supporting piers of masonry are of a distinctly cylindrical shape, and vary in height between 40 and 42·78 metres, and on them will be placed the main girders of the bridge. There will thus be between the lower part of the beams and the level of the sea at low water a free space, varying in height between 61 and 63·78 metres, which height at high water will be reduced to 54 and 56·78 metres respectively (177 feet). This height is amply sufficient for the passage of vessels of whatsoever description or tonnage. By placing the flooring upon vertical cylindrical columns the minimum height of 54 metres is kept throughout the whole width of the span, a result not achieved in the bridge over the Forth. The girders are to be simple, unlatticed, and trussed, so as to insure the proper distribution of all stresses. The level of the permanent way is 72 metres above the low-water level. The height might have been reduced by arranging the permanent way in the lower portion of the bridge, but in that case it would have been necessary to make the cross beams a great deal larger and consequently heavier. By raising the permanent way, on the contrary, a marked economy is attainable which will certainly not be absorbed by increased expenses involved by the necessity of erecting viaducts at both ends of the bridge. There will be a double set of rails, and the width of the flooring proper will be eight metres. The width of the bridge is variable, the greatest distance being between the axes of the main girders, twenty-five metres, a space necessary to insure the stability of the structure under the action of violent gusts of wind. The roadways are of the ordinary width of fifteen metres between the axes and the rails, the latter set in grooves to obviate accidents. The floor, made of ribbed sheet-iron, is to cover the bridge throughout its length, so as to make every part accessible to the men appointed for its supervision. Between and outside of the roadways pavements are provided for the men to stand on, and thus keep out of the way of passing trains. On the flooring may be set up refuges, stations for the guards, signal boxes, switches, etc All these arrangements can be multiplied according to the requirements of the traffic, and scattered over any convenient points and spans of the piers. Lighthouses may be erected to indicate obstacles to be avoided. The various

kinds of lights used in lighthouses may also serve to indicate to shippers the distance from the Colbart and Varne banks. It would have been easy to establish a bridge with four lines of rails, instead of two, but the probable development of the traffic did not appear to warrant any increase of outlay in that direction. A road for ordinary vehicles is also superfluous, as goods will always be carried by rail. To meet objections from a military point of view, arrangements could be made for making the span at either end of the bridge unfit for use; the two end spans, notably, which are in contact with the abutments, might be removable or revolve. The paper proceeded to give a minute account of the mode of construction, and was accompanied by plans. The total length at this site would be nearly twenty-five miles. L. M. H.

CHEMISTRY.

ON THE BEHAVIOR OF WOOD AND CELLULOSE UNDER INCREASED PRESSURE AND AT ELEVATED TEMPERATURES IN THE PRESENCE OF WATER.—The increasing use of pure cellulose from wood in the manufacture of paper, has made it desirable to ascertain how wood withstands higher temperatures and increased pressure, (1) in the presence of water, (2) of dilute acids, (3) of soda solution and (4) of acid sulphate of potassium. And of equal importance was it to ascertain how pure cellulose stood the same conditions, so as to gain some information with regard to the decomposition products of the so-called incrusting substances. H. Tauss has carried out a series of tests upon pure Swedish filter-paper on the one hand and upon fine shavings of beech-wood and fir-wood on the other hand. His results were as follows: Cellulose paper, even of the purest variety, gives, on boiling with distilled water under ordinary atmospheric pressure, traces of sugar. Under increased pressure, the amount of sugar in solution increases, but only at twenty atmospheres' pressure is the cellulose completely hydrolyzed and changed into hydrocellulose, $C_{12}H_{20}O_{11}$. A red coloration of the paper with phloroglucin and hydrochloric acid, the author considers to be due to sugar and to be no indication of the presence of incrusting substances. Wood yields, when boiled with distilled water in open vessels, considerable quantities of soluble matters. The solvent power of water increases notably with increasing pressure, and reaches its maximum at five atmospheres pressure. Above five atmospheres it diminishes again. Under the most favorable conditions, 26·75 per cent. was extracted from the beech-wood and 19·17 per cent. from the fir-wood. Of these amounts, 11·19 per cent. in the first case and 9·07 per cent. in the second case was saccharine material. Besides the sugar, the extracts contain dextrine-like constituents, precipitable by alcohol. From all the extracts of the wood, ether withdraws brownish products of decomposition, which give beautiful color reactions with phenols and hydrochloric acid. Neither aqueous nor ethereal liquids, nor the residues from them, show any vanillin odor or reaction for vanillin. On the contrary, these color reactions all recall Ihl's reaction with phenols and hydrochloric acid, which is obtained with the decomposition products of the carbohydrates. The author therefore concludes that they do not indicate the existence of vanillin

or coniferin in the wood-incrusting matters, but rather indicate the change of woody tissue into carbohydrates and their decomposition products.—*Dingler Polytech. Jour., 1889*, 273-276. S. P. S.

RECENT INFORMATION AS TO THE FAILURE OF THE BAKU PETROLEUM DEPOSIT.—Early in this year statements appeared in the papers as to the gradual failing of the Baku oil wells. These have been confirmed as a result of the careful exploration of the Baku district by A. M. Kouschin, a mining engineer, sent by the Russian government. He states that the Balachone-Sabuntschi naphtha occurrence forms a definite basin, with well-defined boundaries, covering a surface of about fifteen square kilometres. Its boundaries to the south and east are the lines of the out-cropping of the Aral-Caspian limestone, to the west the mud volcano Bog-Boga and its spurs, and to the north the naphtha-containing sediments are interrupted at the farms of the village Zabrat. An overstepping of this naphtha zone has proven futile. The Nobel Company, who bored to the north by Lake Zabrat, experienced absolute failure only. Other firms who bored on the western border of the Balachani district, on the southerly slopes of Bog-Boga, met the same negative results. A boring beyond the southerly or eastern border is also totally without promise, as the naphtha-containing sediments here would be overlaid by such a thick covering of limestone as to make a penetration impossible. So the territory is perfectly defined.

Just as the Balachone-Sabuntschi naphtha zone has a well-defined superficial area, so it is found to have a definite depth, which has been settled by the borings of 1888–89 to be for Balachone, 200 to 300 metres; for Sabuntschi, 300 to 400 metres, and for Romanow, 400 to 600 metres. Below this depth, one strikes a rock which is destitute of light naphtha. The important question, then, is what quantity of naphtha has nature accumulated in this Balachone-Sabuntschi-Romanow basin? The solution of this question carries with it the decision as to when the naphtha of Baku will be exhausted. The Engineer Kouschin, on the occasion of conference with the producers at Baku, estimated that the original quantity held in this basin was four and a half milliards of poods (one pood = 16·4 kilograms). Reckoning the output to date as one milliard poods, there is left three and a half milliard poods. This, Kouschin considers, will last seventeen and a half years, with an annual production of 200,000,000 poods. In case the annual production is pushed, it will come to an end in correspondingly shorter time. The project of a naphtha pipe-line he considers to be entirely impracticable, as the returns for seventeen years would not be sufficient to cover the cost.

The failure of the wells at Balachone, where the deposits lie nearest the surface, and where the strongest oil fountains were found, and the absence of strong pressure at Sabuntschi, where the production is now centred, has produced a crisis in the Baku refining industries and the price of crude naphtha has risen to five kopecs per pood, the highest price for years. At the same time, the price of the stock of the Nobel Company has fallen in the St. Petersburg Stock Exchange. S. P. S.

Correspondence of the Chemiker Zeitung, Sept. 4, 1889.

ON THE PHYSIOLOGY OF TANNIN. From a monograph, *Grundlinien zu einer Physiologie des Gerbstoffs*, von Gregor Kraus. Leipzig, 1889. See also *Journal of the Chemical Society* for September, 1889.—The author describes twenty-one series of experiments, which comprise some hundreds of tannin determinations.

These include the estimation of the amount of tannin in leaves under the various conditions of light, shade and darkness, which lead the author to conclude that light and carbon dioxide are essential agents in the formation of tannin in leaves.

The outer leaves of a plant, exposed to direct sunlight, contain more tannin than the inner leaves.

Leaves which are not green are not capable of producing tannin.

It must not be assumed that tannin is a product of the assimilation of chlorophyll grains, as there are innumerable leaves which assimilate tannin without producing tannin; the oak, willow and alder assimilate in dull weather without increasing in tannin.

The tannin produced in the leaves passes into the branches and roots, and there is no experimental evidence that the tannin which has once passed into the rhizome undergoes further change; there is rather an increase in the amount of tannin in the rhizome through a production in the dark.

The author is inclined to believe the use of tannin to leaves is to protect them, either from being eaten, or to prevent rotting, etc.

Fallen leaves contain as much tannin as they did during their best time of growth, indicating that the leaf tannin is of no value to the plant. During the germination, in the dark, of seeds containing tannin (as oak and horse-chestnut) there is no diminution, but an increase in the amount of tannin. There is not yet sufficient evidence to show whether tannin is produced from non-nitrogenous substances, or whether it is formed in the conversion of nitrogenous compounds into albuminoids. It seems probable that aromatic compounds may be formed in the production of albumen, some of which are used in the building up of albumen molecules, while others yield tannin. H. T.

ON THE DETERMINATION OF THE BOILING POINT OF OZONE AND THE FREEZING POINT OF ETHYLENE. BY K. OLSZEWSKI (*Ann. Phys. Chim.*, 2, **37**, 337, through *Jour. Chem. Soc.*, **56**, 821).—Hautefeuille and Chappuis have shown that ozonized oxygen solidifies to a dark-blue liquid at a pressure of 125 atmospheres, and at the temperature at which ethylene evaporates under the atmospheric pressure, namely, —102·5° C., the ozone remains in a liquid state after the pressure has been reduced to that of the atmosphere, from which it follows that the boiling point of ozone cannot be very much lower than that of ethylene, and, therefore, the author hoped to be able to obtain liquid ozone by injecting ozonized oxygen into a vessel cooled to —150° C. at the ordinary atmospheric pressure. He found, however, that he was unable to do this. The vessel was cooled down to —151° C. by means of liquid ethylene, but no ozone was liquefied, being evidently prevented from doing so by the large quantity of oxygen with which it was mixed, the boiling point of oxygen

being very much lower. By using liquid oxygen at the atmospheric pressure in place of ethylene, the temperature was reduced to —181·4°, and the ozone was then easily obtained in the form of a dark-blue liquid, whilst the oxygen with which it was mixed remained uncondensed and was allowed to escape by an opening at the top of the tube. When the ozonized oxygen was injected into a tube thus surrounded by liquid oxygen at the temperature of 181·4°, a drop of ozone could be observed in the course of a few minutes, and if the influx of gas was then stopped and the oxygen surrounding the tube allowed to evaporate, the ozone remained liquid until the whole of the oxygen had evaporated. It was necessary in performing this experiment to cut off the supply of ozonized oxygen, as when this was not done the liquid ozone was swept out of the tube by the stream of gas. After the oxygen had completely evaporated, the temperature of the tube would be about —150°, namely, that of the liquid ethylene surrounding the tube which originally contained the liquid oxygen.

At the boiling point of oxygen the ozone remained in the form of a dark-blue liquid, which was transparent in thin layers, but became almost opaque at a thickness of 2 mm.

In order to determine the boiling point of the ozone, the tube containing it was removed from the apparatus and placed in another vessel containing liquid ethylene at a temperature of —140°. The ozone was found to remain liquid until the ethylene had nearly reached its boiling point. The temperature at which the ozone began to evaporate was observed by means of a sulphurous acid thermometer, which was found to register —109°, corresponding to —106° of the hydrogen thermometer, so that the boiling point of pure ozone may be taken as —106°.

The greatest care had to be exercised in making the experiments to prevent ozone from coming in contact with the ethylene, which would cause it to explode. In one of the experiments a drop of liquid ozone, just below its boiling point, exploded in this way, and the explosion was so violent that the triple glass walls of the apparatus were blown into fine dust.

When the ozone was allowed to evaporate it changed into a bluish colored gas which retained its color at the ordinary temperature, and could be recondensed by immersing it in liquid ethylene.

In a former paper, the author has described his attempts to freeze ethylene by allowing it to evaporate in a vacuum. His attempts, however, were unsuccessful, the ethylene remaining liquid and transparent at a temperature of —:62°, and a pressure of between one and two millimetres of mercury.

He has now succeeded in solidifying ethylene by enclosing it in a tube surrounded by liquid oxygen, itself surrounded by liquid ethylene, as in the case of the experiments with ozone. The ethylene was found to solidify at about the boiling point of oxygen —181·4°, to a white, crystalline, semi-transparent mass.

When the stopcock, through which the oxygen, as it evaporated, was allowed to escape, was closed so as to allow the pressure and the temperature of the oxygen gradually to increase, the solid ethylene became liquid at

a pressure of 3·4 atmospheres, at which, according to the author's former research, the temperature of the liquid oxygen would be —169°, which may, therefore, be taken as the melting point of solid ethylene. W. H. G.

BOOK NOTICES.

THERMODYNAMICS OF THE STEAM-ENGINE AND OTHER HEAT-ENGINES. By Prof. Cecil H. Peabody. New York: John Wiley & Sons. 1889. pp. xviii, 470.

Most writers on the steam-engine, from a thermodynamic standpoint, determine what would take place in a steam-cylinder if certain conditions were fulfilled, the result being that but little attention is paid to what actually does occur there. The present work, after deducing the general equations of thermodynamics, brings together in a convenient shape many of the reliable experiments that have been made on the steam-engine, so that, as far as our present knowledge extends, the action of steam under different conditions can be studied.

Chapters I to V contain a statement of the first and second laws of thermodynamics and a deduction of the fundamental equations. The discussion of these equations, as applied to perfect gases, completes the sixth chapter. Chapter VII treats of saturated vapor, and gives in a small space an application of the fundamental equation to steam and incidentally to a number of other vapors. The next chapter treats of the thermodynamics of superheated steam. The flow of liquids and the application to injectors is next treated of, the results of experiments with a Sellers, Hancock, Lombard and a Dodge injector being given. Chapter XI treats of hot-air engines, and is descriptive rather than analytical, and gives something of a general idea of the method of handling the problems arising. The subject of gas-engines is barely touched upon.

Chapters XII to XIX are devoted to the steam-engine. The apparatus and methods for conducting tests are given and numerous examples from all sources are given from the experiments of the Naval Engineers on the Michigan, in 1861, to the test of a Worthington pumping engine, by Professor Unwin, in 1888. The apparatus used and the precautions taken are given in many cases. The entire subject is presented in such a way that the data and results can readily be compared and studied.

One chapter treats of the friction of steam-engines, showing what part of the entire friction of the engine is chargeable to any moving part.

Chapters XX treats of compressed air-engines, blowers, etc., and the last chapter deals with refrigerating machines.

The entire work is the best on the subject that has lately been issued, and should be in the possession of everyone studying the action of steam in an engine.

An odd mistake is made on page 183, where the text for that page is omitted and the corresponding page from Whitham's Steam-Engine Design is inserted in its stead. H. W. S.

A MANUAL OF MACHINE CONSTRUCTION FOR ENGINEERS, DRAUGHTSMEN AND MECHANICS. By John Richards. Philadelphia: J. B. Lippincott Company. 1889. pp. 153.

The scope of this work is best given by a summary of the general heads under which the work is arranged. Machine design is treated generally, and then particularly as to sections of machine frames, rib and plate sections, fly-wheel rims, spokes and struts, machine columns and standards, bosses for screws, nuts and foundation bolts, bearings for shafts and spindles and sliding bearings. Section second treats of the transmission of power, driving by belts and ropes, hydraulic, steam, air, gas and electrical transmission. Section third deals with steam machinery and covers the details of valves, ports, engine-frames, crank shafts and pins, connecting-rods, cross-heads and matters connected with steam-boilers. Section fourth treats of hydraulics, water-wheels, pumps, pipes and fittings. Section fifth, of mechanical drawing, heat, dynamics and the properties of materials. The last section treats of weights and measures and gives various tables, such as circumferences and areas of circles, square and cube roots, etc.

It will be seen that the ground covered by the work is quite extensive, and it is surprising that it has generally been so well worked out.

The matter treated of is of the character that comes before everyone engaged in designing machinery, and the book is filled with exactly the details that are wanted.

For instance, take the subject of collars for shafts. The author first states that cast-iron is not a suitable material for solid collars and forged ones are expensive. A sketch is then given of a malleable cast-iron or steel collar, and a table of all the dimensions for different sized shafts is given. The table is followed by remarks in the nature of cautions as to the use of this design. Most of the other subjects are treated in practically the same way.

As long as the author deals with proportions of the details of machinery the advantages of his long engineering training renders that portion of the work very valuable to the designer. When he goes outside of that the work becomes indefinite, and in some cases misleading. For instance, what could be less clear than the following, on indicated and actual horse-power, pp. 77, 78:

"*Indicated horse-power.*—This term is employed to define the pressure exerted by the pistons of engines, and is the mean pressure multiplied by the area of the piston in inches and by the velocity in feet per minute. This gives foot-pounds, that is, pounds raised one foot high in a minute; 33,000 of these foot-pounds are estimated at one horse-power, so that dividing by this number gives indicated horse-power.

"*Actual horse-power.*—From the indicated power of an engine, there must

be deducted certain losses by friction in the engine, friction of transmitting machinery, clearance at the ends of the cylinder and leaks," etc.

That the term indicated horse-power is employed to define pressure exerted by the piston of an engine is new; that foot-pounds mean pounds raised one foot high *in a minute* is not so; and that clearance and leaks are responsible for any of the difference between indicated and actual horse-power is rather surprising.

Again, on the subject of locomotive slide-valves, p. 82, a valve is given with certain data, and the following statement is made: "Steam follows sixty-four per cent. of the stroke. Exhausts at eighty-five per cent. Compression, 0·6 per cent. This applies to the forward stroke." Then, this peculiar statement: "The back stroke will vary as the length of the connecting rods."

Under the head of dynamics (p. 134) we find: "Power is a resultant of force and velocity. Work is a resultant of force, velocity and time. If either force or velocity are changed, power is changed accordingly. If either force, velocity, or space are changed, work is changed in the same proportion."

Definitions of power and work could hardly be put more awkwardly and still be correct.

The form in which the book is gotten up makes it somewhat more convenient than is usually the case with works of this kind. The pages are 6¾ x 10½ inches, bound along the shorter side, which is the top of all the printed pages. Every other page is blank for notes—a most excellent feature. The pages are numbered at the bottom and on both corners, facilitating use. There is no index beyond the table of contents, but as this is quite full very little trouble is required to find any subject.

It is doubtful whether what may be called the general information contained in the work will be of any value to anyone, but as a work of reference for the actual details of machine construction it will be of great assistance to the draughtsman and engineer. H. W. S.

STEAM-ENGINE DESIGN. By Prof. Jay M. Whitham. New York: John Wiley & Sons. 1889. pp. ix, 391.

In the preface, the author states that the work treats of the application of mechanics to the designs of the parts of a steam-engine of any type, or for any duty. While he has given himself a large field to work in, he has covered the constructive details very well. The examples given of the different parts of the engine are from the latest practice. Giving the results obtained by the different methods of treatment adds greatly to the value of the work.

Thus in the early part of the work, under the head of piston, six different methods of building up pistons are given, from the small ones with sprung ring to the largest marine piston. Numerous examples of packing pistons and of setting out the packing are given.

Empirical and analytical methods are given, where possible, for the design of the parts of the steam-engine and the results obtained are usually placed together for comparison. Thus three methods give for the thickness of the piston taken as an example, 4·76, 5·45 and 5·43 inches.

The designs of all the parts of the engine are treated in much the same way, empirical rules being given, and where rational analytical methods can be applied formulæ are deduced and the results then compared with those of actual practice.

Perhaps this can be best illustrated by the design of compound-engine cylinders, as given on page 155. Six methods of deducing the diameters of the cylinders are given, and their results are followed by the actual sizes of the cylinders used. As the engine, if the cylinder were made according to some of the sizes deduced, would cost considerably less than as actually built, it would be interesting to know which method to select if one wishes to design a compound-engine. This is the only defect in the work. Numerous examples or methods of working are given, but the advantages of one method over another, or of one plan of fitting up a part over another, are not dwelt upon to any extent. Ordinarily, the results would vary quite an amount, so that it would often be a serious question in the cost of an engine as to whether one size, or method, rather than another, should be used.

The work, as a whole, cannot fail to be of very great service to designers and draughtsmen, as it brings together many of the methods of working out details, but, as a reference book, it should have been condensed into one-fourth the size.

The work is to be commended for the superior quality of the figures, as much care has evidently been taken in their preparation, and clearer or handsomer ones are seldom found. H. W. S.

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Franklin Institute.

[Proceedings of the Stated Meeting, held Wednesday, October 16, 1889.]

HALL OF THE FRANKLIN INSTITUTE,
WEDNESDAY, October 16, 1889.

Dr. ISAAC NORRIS, in the Chair.

Present, 162 members and thirty-four visitors.

The Actuary's report exhibited six additions to membership since the previous meeting.

The Secretary reported the following resignations from the Committee on Science and the Arts, viz: Profs. Wm. H. Greene and L. B. Hall, and Messrs. J. R. McFetridge, Alex. E. Outerbridge, Jr., and J. Rodman Wharton.

An election to fill the vacancies resulted in the choice of—

Mr. N. H. Edgerton	for the unexpired term of		Prof. Greene.
Dr. Chas. B. Dudley	"	"	" Prof. Hall.
Mr. Thos. P. Conrad	"	"	" Mr. Wharton.
Mr. Wm. H. Thorne	"	"	" Mr. Outerbridge.
Mr. Addison Hutton	"	"	" Mr. McFetridge.

Mr. ALEX. CRAWFORD CHENOWETH, of New York, engineer in charge of the new aqueduct for the city of New York, gave a description of the more important engineering features of this work. The speaker illustrated his subject with the aid of numerous lantern views.

Mr. JULES JUVENET read a paper on "Ramie," with especial reference to the introduction of the culture of this valuable fibre plant into the United States. He gave an account, also, of machinery and processes of his invention for the preparation of the fibre of the plant for use in the textile industries. (Referred for publication.)

The Secretary's Report embraced a description of an improved "car starter," invented by Mr. J. H. Palmer, of Philadelphia, and an account of the process of M. Adolphe Minet, for the production of aluminium by the electrolysis of its fluorides.

Adjourned.

WM. H. WAHL, *Secretary.*